

THE MATHEMATICAL WORK OF

DAVID GREGORY, 1659-1708

Christina M. Eagles

Doctor of Philosophy,
University of Edinburgh,
1977.



I declare that this thesis was composed by me
and embodies the results of my own work.

22nd February, 1977

Abstract

David Gregory (1659-1708) left many manuscripts and from these we can analyse the development of his ideas and his assimilation of Newtonian science.

He was the nephew of James Gregorie (1638-75), a man justly renowned for his skill as a mathematician. David's study of the papers left on James' death led to his interest in integration by infinite series which was the subject of two publications of 1684 and 1688. Already, the influence of what he could learn of Isaac Newton's work was apparent.

In 1683, David became Professor of Mathematics at Edinburgh University, which he left in 1691 to take up the Savilian Chair of Astronomy at Oxford where he remained until his death in 1708 of consumption. In spite of his enthusiasm for Newton's Principia (1687), his Edinburgh lectures were not Newtonian.

In May, 1694, David visited Newton at Cambridge and became one of the early group of Newtonian disciples. He studied Newton's mathematics, and the similar developments being made on the continent. In 1702, with the advice of Newton and his circle, he published his Astronomiae, which was the first astronomy text set in a Newtonian framework.

As a mathematician, David was competent, but not always able to appreciate the new work of Newton and the continental mathematicians. His abilities were better used in expounding the work of others; the long-lasting popularity of his Edinburgh lectures attests their value, and his published and manuscript expositions of Newton's work, though not always free from error, have much to recommend them.

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Preface

David Gregory, who lived from 1659 to 1708 ^{belonged to} was one of the first groups of Newtonian scientists. When Newton's Principia appeared he was already Professor of Mathematics at Edinburgh University, interested in what little he could find out of Newton's mathematical methods. He at once set out to study and master the Principia and was converted to its doctrines. Thereafter, in 1691, he was appointed on Newton's recommendation to the Savilian Chair of Astronomy, Oxford, where he remained for the rest of his life. In England, he became a confidant of Newton whose influence became increasingly predominant in his work. His major text, the Astronomiae, was published in 1702 and was the first astronomy text set in a Newtonian cosmology.

This study is based largely on Gregory's manuscripts of which he left a large number. He was a methodical man and classified and indexed most of his papers into files; quarto A and folios B, C and D. The indexes, at least to A, B and C, were probably drawn up in late 1699 or early 1700, for papers dated after this time have been inserted into the indices after they were completed. This has sometimes helped to indicate the date of a paper. Quarto A originally contained 113 items, folio B 37 and folio C 220. Folio D contained the papers of David's uncle James Gregorie (some of which are also in the other files) but of the original 33 items only 3 are now known to us.

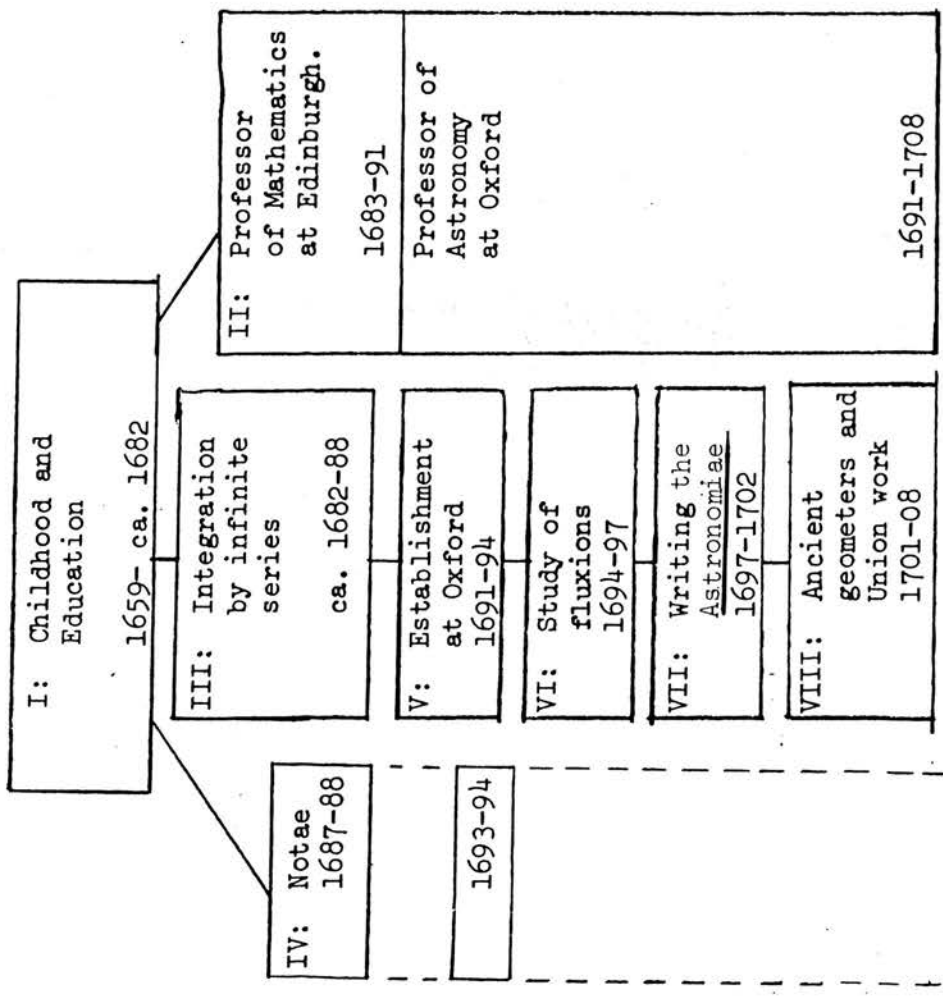
Most of A, B and C are in Edinburgh University Library, but a large number (generally those most closely connected with the work of Isaac Newton) are in the Library of the Royal Society, London. A *

few others are in the Libraries of St Andrews University, Aberdeen University and King's College, Cambridge with one at least in private possession.

These libraries, as well as the National Library of Scotland, Christ Church College and the Bodleian, Oxford, University Library, Cambridge, the British Museum and the Public Record Office hold other Gregory papers [and correspondence to or from him]. In particular, Christ Church, Oxford has workbook E which is primarily a collection of worked examples from the Acta Eruditorum. A large number of loose sheets are catalogued with folio B in Edinburgh University Library, and I have referred to these as 'Misc.'). David Gregory's manuscripts are listed in P.D. Lawrence The Gregory Family (Aberdeen University, Ph.D. thesis, 1971) Appendix 1 316-20. I have given references to individual manuscripts as they arise in the text of the thesis and the bibliography lists the abbreviations I have used in so doing. Manuscript copies of his lecture notes are listed in appendix 1 to chapter 2.

Gregory's correspondence with Newton, and several of his memoranda are published in the first four volumes of The Correspondence of Isaac Newton ed. H.W. Turnbull, J.F. Scott, A. Rupert Hall and Laura Tilling (Cambridge, 1959-). Excerpts from the memoranda in workbook E with a few from quarto A, make up David Gregory, Isaac Newton and their Circle. Extracts from David Gregory's Memoranda 1677-1708 edited by W.G. Hiscock (Oxford, 1937). The lecture which Gregory gave on his appointment as Savilian Professor is published with an introduction in P.D. Lawrence and A.G. Molland 'David Gregory's Inaugural Lecture at Oxford' Notes and Records of the Royal Society

3.6.1659	Born
1671-75	Marischal College
1680-01	Continent and London
ca. 1682	Began to study James Gregorie's papers
1683	Edinburgh appointment
1687	Newton's <u>Principia</u>
1690	Visitation committee
1691	Oxford appointment
1692	F.R.S.
May, 1694	Visit to Newton
Winter, 1696-97	Catenary, brachistochrone
1703	Edition of Euclid
10.10.1708	Death



XXV (1970) 143-178. The work of David's uncle, James Gregorie, is discussed extensively in James Gregory Tercentenary Volume edited by Herbert Westren Turnbull (London, 1939). This volume contains James' correspondence with John Collins. A letter of David Gregory to Christian Huygens is in Oeuvres Complètes de Christiaan Huygens publiées par la Societe Hollandaise des Sciences, 10 (Correspondance, 1691-95) (La Haye, 1905) 471-73. David Gregory's manuscripts were used in Stephen P. Rigaud Historical Essay on the First Publication of Sir Isaac Newton's Principia (Oxford, 1838) which published one of Gregory's manuscripts as appendix 23, pp 79, 80. A letter from Adrian Verweer to David Gregory is in S.P. Rigaud Correspondence of Scientific Men of the seventeenth century 2 vols. (Oxford, 1841) i 248-53. David Gregory's papers have been used, some published and many cited from, in The Mathematical Papers of Isaac Newton, edited by D.T. Whiteside (Cambridge, 1967-). Publications of manuscripts are given where these manuscripts are cited in the footnotes.

From these manuscripts, a detailed picture of Gregory's life emerges. We can roughly classify it into eight sections as shown in the diagram opposite.

Section I consists of his early days as schoolboy and student. It can be taken to end some time in 1682 (or perhaps 1681) when, on his return from the continent and London he began seriously to reconstruct the work of his uncle, James Gregorie, from the papers left on his death.

Section II runs concurrently with most of the others and begins in 1683 with Gregory's appointment as Edinburgh University's Professor of Mathematics. By 1691 political considerations made it

wise for him to leave Edinburgh and on Newton's recommendation he was appointed Savilian Professor of Astronomy at Oxford, which post he held until his death. The lectures he gave, especially at Edinburgh, form a large part of this study.

Section III is the study of integration by infinite series which David carried out. The possession of his uncle's papers on this topic was the deciding factor in choosing this direction of research. The Exercitatio (1684) and a passage in Pitcairne's Solutio (1688) arose out of this study. Gregory's research continued for some months after publication of the Solutio, but town and College politics soon absorbed him.

Section IV, the Notae, Gregory's commentary on the Principia, was never really finished. There were two main phases of composition, 1687-88 and 1693-94; the first was instigated by the appearance of Newton's book and the second probably arose when Gregory was settled in Oxford with time to work on the Notae again. However, Gregory continued to add to them for the rest of his life.

Section V followed Gregory's move to Oxford. He did not see Newton during this time, and concentrated on establishing himself in the English scientific world. In 1692 he became F.R.S. and in 1694 published his first paper in the Transactions. He met Christian Huygens when he journeyed to Holland in 1693.

Section VI, study of fluxions, followed Gregory's crucial visit to Newton in May, 1694. This visit marked his return to Newton's favour, and for the rest of his life Newton's influence would predominate. His study of fluxions began with a tract on Newton's methods which he followed with a study of many examples from the Acta. Although

they were not quite the last problems he tackled in this field, the catenary and the brachistochrone were a climax to this period, for here Gregory attempted to apply in his own work what he had learnt from the study of others.

Section VII is the Astronomiae. Gregory's reasons for starting this work are discussed in detail in 5.1.1; broadly, he had lost hope that his Notae would be published, and this was in some ways a substitute.

Section VIII begins before the publication of the Astronomiae in 1702. By the summer of 1701 Gregory had already begun to plan his edition of Euclid which appeared in 1703. Thereafter he was busy with a projected edition of Apollonius, but he died before this was complete. This work may have been a reflection of Newton's interest in the Ancients: more certainly, it was undertaken at the desire of the University. Gregory's final years were also busy with government work involved with the Union of Scotland and England. Newton's influence probably helped to win him the task of overseeing the recoinage at Edinburgh Mint, and Gregory may have hoped that this would lead to a government position in London.

The chapters of this thesis largely follow the pattern I have outlined above. Chapter one looks at Gregory's life in full and includes especially sections I, IV and VIII. Chapter two considers Gregory as a teacher and corresponds to section II. The third chapter deals with the mathematical research on which Gregory was involved at Edinburgh; section III on integration by infinite series. Sections V and VI make up chapter four, and section VII is chapter five. Finally, chapter six consists of some concluding remarks, or

the nature of Gregory's abilities.

When he moved to England in 1692, David abandoned his family's spelling of their surname, Gregorie, in favour of the Anglicized Gregory by which he is generally known. The rest of his family retained the Scottish spelling in this period, and I have used it in referring to them, using 'Gregory' for David alone. This not only accords best with their own usage, but helps to avert an inevitable confusion between the various members of the family.

I have given dates in the old style used by Gregory; that is, those based on the Julian calendar which supposed the length of a year to be precisely $365\frac{1}{4}$ days. This gave dates 10 days behind that of the new style, or Gregorian, calendar already in use in most continental countries at that time. (That is, the 1st July, old style, was 11th July, new style.) However, the Julian year began on 25th March and dates between 1st January and 24th March were normally denoted by a double year. ~~Thus, February 169 $\frac{1}{2}$ was in 1691 old style, but in 1692 new style.~~ I have not used double years, but have used new style throughout for the years so that I have written February, 1692 for the February 169 $\frac{1}{2}$ which Gregory and his British contemporaries would have written.

Abbreviations of texts used in the footnotes are to be found in the Bibliography, ~~p.~~ at the end of this thesis

Acknowledgements

I would like to thank the Royal Society of London for a grant towards the costs of microfilming manuscripts. The librarians of all the libraries I have mentioned in the Preface have been unfailingly courteous and helpful, especially Mr Finlayson of the manuscripts department and the rest of the staff of Edinburgh University Library and I would like to express my appreciation to them all. I am extremely grateful for the help and advice Dr D.T. Whiteside of Cambridge University has given me, especially with the brachistochrone and other aspects of Gregory's work which were closely associated with Newton's mathematics. Above all, I wish to thank my supervisor, Dr Eric G. Forbes of Edinburgh University for his patience and guidance over the $3\frac{1}{2}$ years it has taken to complete this thesis. Finally, I would like to thank my parents for their invaluable encouragement and support throughout this period.

Chapter 1

The Life of David Gregory

David Gregory was born in 1659 into an Aberdeen family already known for its academic abilities. After an education at Marischal College, Aberdeen, he spent some time abroad before his appointment in 1683 as Professor of Mathematics at Edinburgh University. He stayed in this post until 1691 when, partly because of the activities of the 1690 visitation committee, he left to take up the Savilian chair of Astronomy at Oxford. At Edinburgh he lectured on a wide range of topics and published two works on mathematics. The first, which appeared in 1684 was mainly a reconstruction of the methods used by his uncle, James Gregorie. The second was a shorter piece on integration by series which appeared in a tract by Archibald Pitcairne, an Edinburgh physician and lifelong friend of Gregory's.

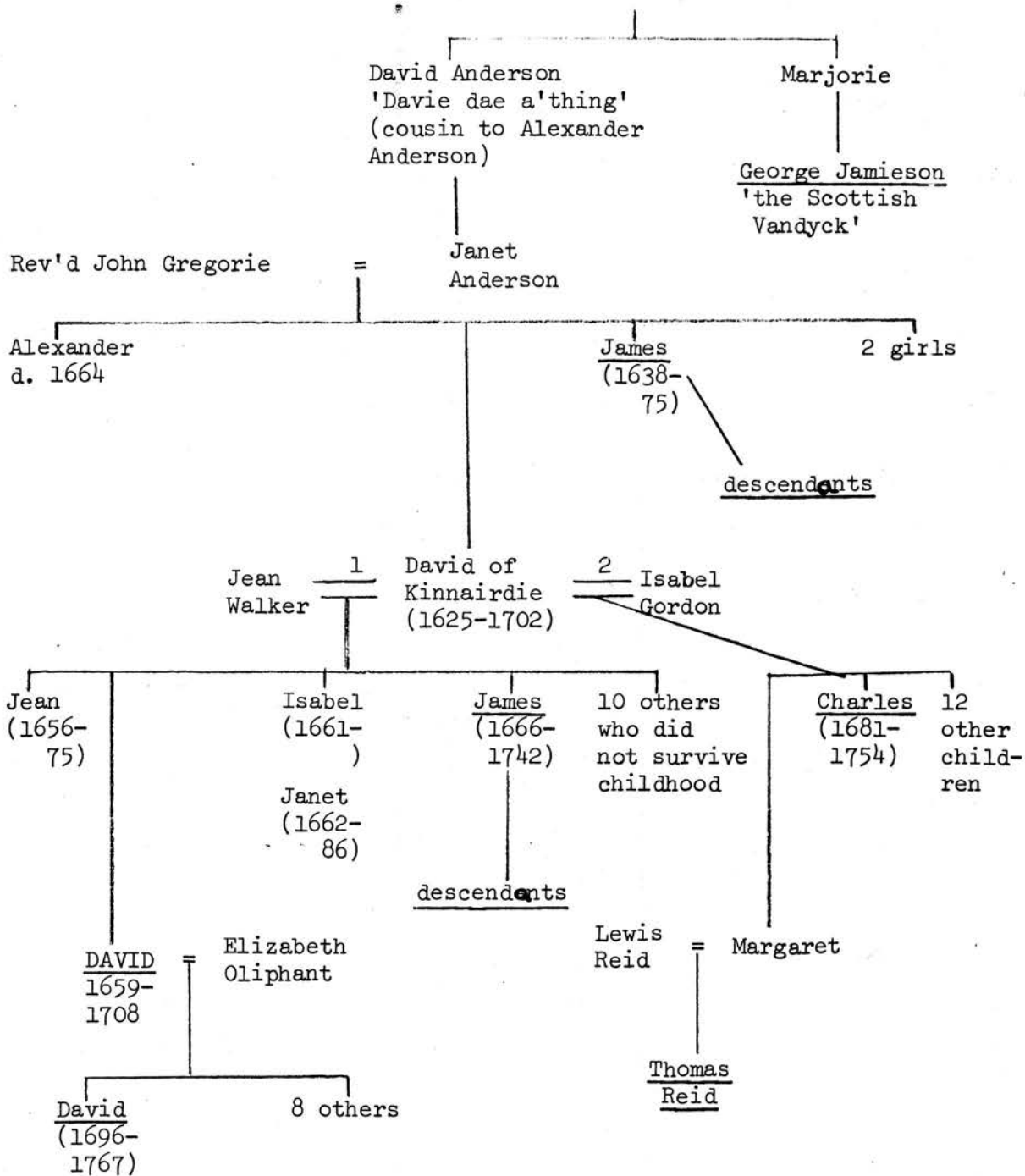
Newton and Flamsteed helped him to win the Oxford chair and Gregory joined the Royal Society in 1692. Thereafter he contributed several papers to the Transactions. In 1695 he published a work on Optics, taken largely from his Edinburgh lectures, in 1702 an astronomy based on Newtonian principles and in 1703 an edition of the works of Euclid. His friendship with Newton developed during the years at Oxford, and Newton's influence is found in almost all his work. He also completed the notes on Newton's Principia which he had begun at Edinburgh and in 1694 wrote a tract on Newton's method of fluxions. He was also friendly with Arthur Charlett and Edmond Halley, but his relationships with John Flamsteed and Thomas Hearne were not so happy.

In 1695 Gregory married Elizabeth Oliphant of Langton and they had

nine children, one of whom became Oxford's first professor of Modern History.

The last years of Gregory's life were busy with work for the Union, notably in calculating the Equivalent to be paid to Scotland and in overseeing the Edinburgh Mint. This work, coupled with preparations for a projected edition of Apollonius and Serenus proved too much for his health. In October 1708, Gregory died in Maidenhead of consumption.

Several posthumous works followed his death. In 1734, an excerpt from his mechanics lectures appeared in Martyn and Eames' abridgement of the Transactions. A tract "De stellarum ortu et occasu poetico" was included in the 1743 edition of Manilius' Astronomy. In 1745, Colin McLaurin published a translation of Gregory's lectures on practical geometry. These are detailed with Gregory's other publications in Appendix one.



Those underlined all achieved at least recognition in their fields, generally mathematics or medicine. This includes many descendants of both James'.

1.1 Family Background

David Gregory was a member of a family renowned for its abilities in mathematics and medicine¹. Even before his time two of its members had made a name for themselves as mathematicians and both his father and the latter's maternal grandfather were well-known figures in Aberdeenshire. David and two of his brothers were professors of mathematics at British Universities, and future generations produced many more professors of mathematics and medicine as well as the philosopher, Thomas Reid. (See family tree opposite.)

The Gregorie family's ancestry can be traced back to Gregor MacGregor of Glenlyon in the fifteenth century. The change to Gregorie occurred in the sixteenth century before the name McGregor was proscribed in 1603. The mathematical abilities apparently entered the family, as Galton pointed out, with the marriage of the Reverend John Gregorie to Janet Anderson in 1621². Janet's father David, of Finzeach in Aberdeenshire was known locally as "Davie dae a'thing". He was a well-to-do man with a practical turn of mind whose achievements included removing a large stone from the mouth of Aberdeen harbour and designing the steeple of St Nicholas church. His cousin, Alexander Anderson, was a professor of mathematics at Paris in the

¹ For the Gregory family see P.D. Lawrence The Gregory Family: A biographical and bibliographical study. (Aberdeen University Ph.D. thesis, 1971).

Also John Gregory A Father's Legacy to His Daughters. (Edinburgh, 1788) which has a biography of the author as preface.

Also Agnes Grainger Stewart The Academic Gregories. (Edinburgh, 1901).

Thomas Reid, the philosopher, who was our David's nephew, gives additional details in The works of Thomas Reid, D.D. ed. William Hamilton. (Edinburgh, 1846) 68-70.

² Francis Galton Hereditary Genius edition 2. (London, 1892) 206.

early seventeenth century, who edited Viète's posthumous works, as well as publishing on his own account. Through David Anderson's sister the family were also related to the painter, George Jamieson, known as the Scottish Vandyke.

The Reverend John Gregorie was minister of Drumoak on the Dee and as an opponent of the Covenanters his position was a difficult one. Twice he was deposed and reinstated. Meanwhile, he had inherited land through his wife, and the estates of Kinnairdie and Netherdaill in Banffshire came to him as settlement of a debt. He and Janet Anderson had three sons, Alexander, David and James, and two daughters Margaret and Janet. Alexander inherited his father's estates in 1650 but he died childless in 1664, murdered by the family to whom the estates had first belonged. They then passed to his brother David who is generally known as David of Kinnairdie (1625 - 1720), who was our David's father.

This David had begun life apprenticed to a merchant house in Holland, but on his father's death he returned to Aberdeen and apparently devoted himself to scientific and literary pursuits. He began a correspondence with the French scientist Edme Marriotte around this time. On Alexander's death, David found himself a rich land-owner. He is said to have been laughed at by his neighbours for his ignorance of farming. His interests lay rather in mathematics and medicine, the traditional fields of the Gregorie family. He had no degrees, but he practised medicine locally free of charge for all who wished his services. His brother James testified to his abilities as a mathematician and the papers of his son David contain some of his work on topics such as Diophantine equations and parallax.

On 26th July, 1683, shortly before his son's appointment to the Edinburgh chair of mathematics, he was appointed Justice of the Peace³.

His first marriage was to Jean Walker, daughter of Patrick Walker of Orchiston, an Aberdeen merchant. She held strong Episcopalian and Tory views which were traditionally shared by all her children, while those of the second marriage followed their mother in being staunchly Presbyterian and Hanoverian. Jean Walker died in childbirth in October, 1671 and barely four months later on 15th February, 1672, Gregorie of Kinnairdie made ~~his~~ this second marriage to Isabel Gordon, also the daughter of an Aberdeen merchant. These marriages gave David of Kinnairdie 29 children; fifteen by his first wife and fourteen by the second. Not all survived childhood; of the first marriage two sons, including our David, and four daughters grew to adulthood, but two of the girls died unmarried aged nineteen and twenty-four. Four sons and three daughters of the second marriage survived.

David of Kinnairdie's younger brother James was perhaps the most important of the family⁴. He was born at Drumoak in 1683, and educated at home by his mother and elder brother. He attended Aberdeen grammar school and Marischal college, from where he graduated in 1657. One of his classmates at University was Gilbert Burnet, later Bishop of Salisbury. His first work was the Optica Promota (London, 1663) which he wrote in Aberdeen under the encouragement of his elder

³ Register of the Privy Council of Scotland viii (1683-84) 3rd series. (Glasgow, 1915) 200.

⁴ For James Gregorie's life and work see GTV.

brother. This work contained an independent derivation of the "sine law" of refraction and the design of a reflecting telescope, still known as the "Gregorian telescope". From 1664-68 James travelled on the continent. He visited Flanders, Rome and Paris, but most of his time was spent at Padua. There he studied with Riccioli, Manfredi and degli Angeli through whom he learnt the methods of indivisibles proposed by Cavaleiri. In Padua he published his Vera Quadratura Circuli et Hyperbolae (Patavii, 1667) and Geometriae Pars Universalis (Patavii, 1668). The first was a bold attempt to prove that it is impossible to express the area of elliptic, hyperbolic or circular sectors as finite combinations of the elementary arithmetic operations. That is, it attempted to prove that π is transcendental. His argument was fallacious, but it was a stimulating work which introduced many new concepts. In particular he was the first to apply the term "convergent" to infinite series though his usage was rather different from that current today. The second book was less original, but was the first attempt to write a systematic text book on the calculus. As a sequel he published on his return to Britain Exercitationes Geometricae (London, 1668) which contains many results on logarithmic and trigonometric functions.

In 1669, James came to St Andrews as their first Professor of Mathematics, a post which he held until 1674 when he accepted an offer of the mathematics chair at Edinburgh. Tragically, he died in 1675 after a short illness. At St Andrews he published his last work; a tract appended to Patrick Mathers' The Great and New Art of Weighing Vanity (Glasgow, 1671). This deals mainly with vibratory motions of particles on a vertical circle. However, his other research work has

not been lost to us. He kept up a correspondence with John Collins in London, who sent him news of the latest advances in mathematics, including some of the work Isaac Newton was then doing with infinite series. Gregorie wrote back to him sending results of his own, and used the blank spaces of the letters he received from Collins and others for his calculations. From these papers the late Professor Turnbull has reconstructed his work which included the discovery of the series generally attributed to Brooke Taylor. On his death, his papers passed to his brother of Kinnairdie and thus to David, the subject of this thesis.

1.2 Early Life

It was into this family that David Gregory was born on 3rd June, 1659, at 2.10 a.m., in Upper Kirkgate, Aberdeen⁵. His godfathers were Gilbert Mollison, David Sinclair, George Wilson, Robert Burnet and Walter Melville⁶. He was the fourth child of his father's first marriage but only Jean, three years older than himself, was still alive. His sisters Isabel, Janet and Christian were born in the next five years and his brother James in 1666. Two years before James' birth, when David was five, their father inherited Kinnairdie and the family moved from Aberdeen.

We have no details of his childhood or elementary education, although it is generally supposed that he studied at Aberdeen Grammar School⁷. In 1671, when he was barely twelve years old, David entered Marischal College, which he attended till 1675⁸. His regent was Robert Patersoune, son of John, Bishop of Ross. He had himself been admitted to the first year of his studies at Marischal in 1661, and so was probably only ten or twelve years older than David. He had become a regent in 1667, and on 21st November, 1678 he was appointed Principal of the College⁹. Unfortunately, we have no record of his

⁵ C195, AUL MS 2206 48 B1; P.D. Lawrence & A.G. Molland "David Gregory's Inaugural Lecture at Oxford" Notes and Records of the Royal Society 25 1970 143-178, 144.

⁶ Lawrence op cit¹ 22.

⁷ e.g. Lawrence and Molland op cit⁵ 144, Stewart op cit¹ 52; David Irving Lives of Scottish Writers (Edinburgh, 1839) 2 volumes ii 242.

⁸ Fasti Academiae Mariscellanae Aberdonensis ed. P.J. Anderson. New Spalding Club Publication (Aberdeen, 1898) 239, 240, 242, 244.

⁹ Ibid 28, 37.

teaching, but we may find an indication of his popularity in the enrolment figures for Marischal, which show an unusually large intake for 1671. In the ten years from 1666 to 1675 an average of 30 boys enrolled each year, but in 1671, 54 new students were admitted¹⁰. Since the regents operated a rotating system each student was taught by the same man throughout his four years at university, and this man was chosen by the year of enrolment. Also, students generally entered university at fourteen, and it may be that David was sent two years earlier than usual so that he might benefit from Patersoune's teaching.

Duncan Liddell had been professor of mathematics since 1661. He is said to have taught geometry, navigation and gunnery in London before taking up this post in which he was succeeded by his son in 1687. He too, had been educated at Marischal College, which he had entered in 1603¹¹. Unfortunately, he is also an unknown quantity and we cannot evaluate his influence on David.

The Universities of Aberdeen were generally held to have Jacobite and Episcopalian sympathies, although of the two colleges King's was originally Episcopalian while Marischal's was Presbyterian¹². Cartesian ideas entered the Scottish Universities in the 1650's, but it was only in the late 1670's that they won any general acceptance¹³. By the

¹⁰ Christine Shepherd Philosophy and Science in the Arts Curriculum of the Scottish Universities in the Seventeenth Century (Edinburgh University Ph.D. thesis, 1975) 409.

¹¹ Officers of the Marischal College and University of Aberdeen ed. P.J. Anderson (Aberdeen, 1897) 53.

¹² Shepherd op cit¹⁰ 316.

¹³ Ibid 211.

1660's, though, most regents accepted the Copernican, heliocentric, cosmology¹⁴. Thus David's University education probably exposed him to no more strongly held Episcopalian views than he heard at home. His teaching almost certainly included a Copernican cosmology, which may have been set in a Cartesian framework. Even if Descartes' ideas were dismissed he must have heard them discussed.

However, there cannot have been much modern teaching in science at University which David could not receive at home. His father was self-taught but interested in modern scientific developments. His correspondent, Marriotte, was a prominent Cartesian scientist. More important, David's uncle, James Gregorie, was then teaching mathematics at St Andrews and Edinburgh. He was in touch with the latest research through John Collins and most probably kept his brother at Kinnairdie in Banff well informed. It seems unlikely that anyone at Marischal College had the opportunities which the Gregories had of keeping up with the mathematical developments. Perhaps we can obtain a hint of David's studies at Marischal from his later comments on Scottish education. He complained that not enough time was spent on mathematics, but instead the best years of a youth's life were trifled away on philosophy¹⁵:

David, although he studied at Marischal for four years does not seem to have graduated from there. He probably then returned to his father's home of Kinnairdie. Several changes had taken place while he was at University. His mother had died soon after he entered Marischal

¹⁴ Ibid 298.

¹⁵ C 187 written in 1687. *ms. The MS is in CUS.*

and his father had swiftly remarried. A family of step-brothers and step-sisters was growing up. In 1675 David's elder sister Jean had died and of course, his uncle, James Gregorie, died in October of that year.

We cannot tell if David resented his father's swift remarriage or his step-mother's influence on him. Traditionally, animosity sprang up between the children of the first marriage and Isabel Gordon, Kinnairdie's second wife. In 1690, Gregory was involved in political and financial juggling with the committee appointed to visit Edinburgh University. Lord Raith had supported him, but his patience was wearing thin as Gregory continually excused himself from swearing allegiance to the Hanoverian crown. He explained to Raith

'that my father was ane old man with a second wife who would take advantage of me in case of such behaviour that I entreated time untill I might settle affairs with him'¹⁶.

This may have indicated a bitter struggle with his step-mother, or it may have been simply an excuse for Raith's benefit. David had other good reasons for not wishing to swear to the oath, but Raith would not have sympathised with these. It may have been as a result of David's settling affairs with his father that the estate of Kinnairdie was signed over to him in that year - an action which is hard to reconcile with a situation in which son and step-mother were permanently at odds!

¹⁶ B 26.

However, whatever relations may have been later it seems that they were not intolerable in June, 1675, when David, aged 16, left Marischal College. He probably spent most of the time until his appointment to the Edinburgh Chair of Mathematics in 1683 at his father's house. Certainly he spent the last two years of this period there. Kinnairdie inherited his brother James' papers and books in October, 1675, and perhaps David tried at once to understand his uncle's books. However, we have no evidence that he did so before his return in 1681 from his travels to the continent and to London.

We may assume that by 1680 David had acquired, either at University or from his father, the knowledge which he would later present as the basis of a mathematical education. (See chapter 2.4.2). He would have a firm grounding in arithmetic and a sound knowledge of Euclid's Elements, books 1-6, 11 and 12. He would understand the basic methods of trigonometry and surveying and probably know something of logarithms and elementary algebra. His education would also have included some amount of astronomy, optics and mechanics.

One of the earliest examples we have of David's work is a set of notes on Dechaies' Cursus Mathematicus¹⁷. They examine the statics of weights on inclined planes, the lever and the variation in a barometer with altitude. This paper includes attempts on several related problems, one of which is marked as having been solved altogether on 31st January, 1679¹⁸. This probably refers to 1680, new style, but in

¹⁷ Claudius Dechaies Cursus seu Mundus Mathematicus 3 vols (Lugdini, 1674).

¹⁸ C 117.

any case, we can say that Gregory studied Dechaless' work in the late 1670's.

This book was an encyclopaedic work in three volumes covering all branches of mathematics including mechanics, optics and astronomy along with music, architecture, hydrostatics and geography as well as many other related topics. David was later to use the work for his own lectures on mechanics. John Collins had mentioned its forthcoming publication several times in his letters to James Gregorie, and when it did appear he gave a good account of it¹⁹. He was careful to advise his friend that the book-seller had sent three copies to Edinburgh²⁰ and Gregorie may well have bought one of these. On his death it would have gone to Kinnairdie and so have been available to David on his return from University. If he studied the work thoroughly, he would have received the basis he needed in these general mathematical topics. It also included a section on the geometry of indivisibles and another on algebra, though Collins had felt this last was the weakest point in the book²¹.

In 1680, David was sent abroad to complete his education and it may have been only then that he was introduced to the geometry of Descartes. The Latin edition of Descartes' Geometria which Schooten brought out in 1659 included notes by de Beaune, Hudde, Heuraet, de Witt and himself. David's study of this edition is evidenced in his many notes on it, and of the 17 such papers which we have, all of a similar appearance, 4 are dated 1680 while the others are undated.

¹⁹ GTV 293.

²⁰ Ibid 295.

²¹ Ibid 293.

It seems probable that they all date from this year. By May, 1680, his studies had progressed to the point where he could write from Rotterdam to a friend explaining certain difficulties in Descartes²². James had had a copy of the work²³, but perhaps it had not passed to Kinnairdie with his other books. Or perhaps it was only under the stimulus of hearing Descartes' work discussed by the mathematicians he met on the continent that David was led to a serious study of the book. Perhaps it was simply that only when he was abroad did David's mathematics mature to the point where he could usefully study Descartes.

His interest in Cartesian mathematics was first caught by their use in resolving problems of classical geometry. However, this soon led him to more complex situations and the problems of tangents, of maxima and minima and of rectification. He became acquainted with Descartes' method of tangents, and Hudde's method for maxima and minima. Through the writings of Renaldini he became acquainted with what were basically Fermat's methods for these operations²⁴.

However, these topics were not all Gregory studied abroad. In March, 1680, he was in Leyden, where he made notes on probability in dice-throwing, drew a plan of Descartes' house and sketched a magic lantern²⁵. He was in Rotterdam in May²⁶, and by August in Paris. There, as well as continuing his studies in mathematics, he spent some

²² C134; David's other notes on this edition are A48, A101, C3, C99, C52, A51, A63, A66, A65, C150, C103, C135, A71, C88, C2, C148.

²³ GTV 367. for example, refers to it.

²⁴ C5; C8.

²⁵ C153, C154, C159.

²⁶ C134.

time at the Observatory, where he sketched a secret chamber, a quadrant in use there, and Römer's 'celestial spheres'²⁷. In December he was still in Paris, sketching a barascope, a gravometer, a spirit level and an hydraulic machine which caused a doll to dance up and down in a bottle²⁸.

In May and early June, 1681 he visited London, and here also he made notes on many strange and curious things. He saw Boyle's pneumatic pump, a water siphon and a method of making 'leaves' by dropping molten green glass into water. A Mr Lamb talked to him about engraving on copper, and he visited Gresham College where he saw Newton's reflecting telescope²⁹. Again his mathematical pursuits were not neglected and on 2nd June he copied out a paper of Girard's on equations³⁰. On 4th May Sir Christopher Wren, as President of the Royal Society, gave him permission to attend a meeting. There he heard the question of a pendulum's motion in a vacuum discussed³¹.

We do not know who Gregory met on these travels, or who introduced him to the Paris Observatory, or the Royal Society. Certainly, the reputation of his uncle and probably his father's letters of introduction must have made these things possible. It is unlikely that he met Huygens on this occasion, for the Dutchman was ill at this time,

²⁷ C159, C165.

²⁸ C157.

²⁹ C9.

³⁰ C18.

³¹ Thomas Birch The History of the Royal Society of London (London 1756-7) 4 84.

although they may have met briefly in Paris in December, 1680³². More probably, as is argued in Chapter 3, Gregory would have called on John Collins in London. However, apart from his diligent records of the marvels he saw, and the development of his mathematical interests, we know little of Gregory's first visit abroad.

Thereafter, he settled at home for some years, and began to tackle his uncle's papers in earnest. His mathematical knowledge was broadening now and he had the confidence to submit a paper to the Royal Society on Sluse's method of tangents. Unfortunately, this paper, discussed in 3.4, was not of a high standard, and the Society seems to have swiftly forgotten it!

It was in 1683 that Gregory received notice from Edinburgh University of his appointment to the Chair of Mathematics, which was made on the 17th October in that year³³. He said later that he had spent most of the previous two years at his father's home in Banff, and, in particular, had been there for five months prior to his election³⁴.

³² A.E. Bell Christian Huygens (London, 1947) 80, 82.

³³ Andrew Dalzel A History of the University of Edinburgh (Edinburgh, 1862) 199.

³⁴ B25.

1.3 Archibald Pitcairne

Gregory's appointment to the Edinburgh chair was largely based on his uncle's reputation. On James' death, the chair had been left vacant, and the teaching of mathematics had been taken over by one John Young, who was in January, 1676 allowed an annual salary of 300 marks, later increased to 400³⁵. Young held this post until David's appointment, but was never made professor.

Young's teaching did not give general satisfaction and it was thus, in March, 1683, that we first find the poet-physician, Archibald Pitcairne, in Gregory's life. Among David's papers there are two copies of two broadsheets of Pitcairne's, the first dated 1st March, 1683³⁶. This publicly challenged Young to solve two problems; one concerning the arithmetical manipulation of surds, and the other on raising a multinomial to an unknown power. Pitcairne allowed a month for an answer, but on 20th March he produced a second sheet. Young had erred in the first one, pronouncing the quantities to be surds, when they reduced to whole numbers, whence, Pitcairne commented, it was clear

'what an unlucky and ungeometric guess this Master of
Chance, not of Arts, has made.'

Young returned no answer to the second question. Pitcairne referred to Descartes' Epistolae for the problems. It seems highly likely that

³⁵ Dalzel op cit³³ 199.

³⁶ C187 and C196.[^]

this challenge, or a campaign of which it was part, led to Young's replacement by David Gregory some seven months later.

We do not know when Pitcairne and Gregory first met. Certainly they were not, as Reid suggests³⁷ undergraduates together. Pitcairne was born in 1652 and graduated from Edinburgh in 1671 when David was only beginning his studies at Aberdeen. They may have met in Paris in 1680, for Pitcairne travelled to France in 1675, where his interest in medicine developed. He took the degree of M.D. at Rheims in August, 1680 and soon afterwards returned to Edinburgh but he may have spent some time in Paris on the way home. Perhaps Gregory was in the habit of visiting Edinburgh on occasions in the next few years and the acquaintance ripened there. However, there is no evidence that they knew each other before Gregory took up the Chair, or that David was in the habit of visiting Edinburgh, a long, arduous journey from Banff.

Gregory informs us that for five months before his election, that is, from May, 1683, he was at his father's home³⁸. However, this would have allowed him to have been in Edinburgh at a time when he might have helped in, or even instigated Pitcairne's challenge to Young.

The date at which the friendship began is important because it is frequently stated that Gregory's influence led Pitcairne to study

³⁷ Reid op cit¹.

³⁸ B25.

mathematics³⁹. As a physician, Pitcairne was to become a leading figure of the iatromechanist school, whereby the principles of mechanics are applied to the body. Special emphasis was laid by the early iatromechanists on the circulation of the blood, and one of Pitcairne's earliest works concerned this phenomenon and Harvey's priority in discovering it⁴⁰.

Although I have found no primary evidence in support of this claim it may well be true that, if the two men met before Gregory's appointment to the Edinburgh Chair, Pitcairne's interest in mathematics was fired by Gregory's. However, if, as seems more likely, their acquaintance, or at least their friendship, dated from October, 1683, it is now clear that Pitcairne was interested in mathematics before he met Gregory. These broadsheets display a knowledge of mathematics, a familiarity with the mathematical work in Descartes' *Epistolae*, and, most importantly, Pitcairne's confidence that he was a better mathematician than the University's mathematics teacher. Friendship with Gregory may have helped Pitcairne keep in touch with the most recent mathematical developments, and certainly provided him with a fellow enthusiast. Nevertheless, it seems most probable that Pitcairne was an enthusiastic amateur mathematician before he met Gregory.

However, this may have been, the two shared their mathematical interests in the 1680's when they were both in Edinburgh. Significantly

³⁹ For example, the article on Pitcairne in D.S.B. states that in 1680-81 Pitcairne 'returned to Edinburgh and was stimulated by his close friend David Gregory to take up mathematical studies, which he pursued with verve and some ability'.

⁴⁰ Archibald Pitcairne *De Inventoribus Rerum ...* (Edinburgh, 1688).

perhaps, there is no sign that Pitcairne collaborated in Gregory's Exercitatio which he wrote in the years before coming to Edinburgh and in his first year there. Notably, though, they worked together on Gregory's 'second method' of quadrature between 1685 and Gregory's removal to Oxford. This method was first published in Pitcairne's tract de Inventoribus⁴¹.

Any influence between them seems more likely to have been the development of Gregory's interest in medicine. His leanings toward the iatromechanical school are clear in the speeches he gave in 1692 for the Oxford degree of M.D. (See 2.6.2). Gregorie of Kinnairdie was also a keen physician and David's interest probably arose first at home, but it was almost certainly Pitcairne who steered him towards an iatromechanical interpretation.

When Gregory left Edinburgh in 1691, the two kept up a correspondence and in 1693 Gregory visited Pitcairne in Leyden where he was * then teaching. We have only a few of their letters now, mostly Pitcairne's containing medical hints which are in his 'Specimena Praxeos' a notebook of prescriptions which Gregory helped him set in order in the early eighteenth century⁴². Pitcairne's letters to Colin Campbell generally contain news of Gregory⁴³. In the early 1700's, David was travelling to Edinburgh on business for the Union and their friendship certainly revived then. Most of this evidence of correspondence dates from this period and it was almost certainly

⁴¹ Ibid.

⁴² Dc. 1.62.

⁴³ CCC.

through Pitcairne that Gregory was on 22nd August, 1705 elected an honorary fellow of the Royal College of Physicians at Edinburgh. He took his seat on the board on 4th October⁴⁴.

Pitcairne was a man of ribald wit who used his talent to promote the Jacobite cause and to poke fun at the Protestant divines. His Latin verse has been highly praised, and, while his views were very extreme, he made notable contributions to medicine. He was a very generous man, and a friendly one, and, although involved in many disputes 'He loved his friends, and laughed at his enemies'⁴⁵. He had the reputation of a heavy drinker and an atheist, but the last, at least, was probably unjust.

This man, then, was Gregory's close friend at Edinburgh, and the friendship survived throughout Gregory's life. Pitcairne was especially constant in his support during his friend's troubles with the 1690 committee of visitation. (See 1.6.)

⁴⁴ Irving op cit⁷ ii 261.

⁴⁵ A remark of Sewell's, quoted ibid ii 217.

1.4 Life at Edinburgh

Following his appointment as professor of mathematics, Gregory came to Edinburgh and on 27th November was granted the degree of M.A. by the University, although he seems never to have studied there. He signed the graduation book with the initials M.P., for 'Matheseos Professor' after his name⁴⁶. On 10th December he delivered his inaugural lecture to the University⁴⁷.

Here he began by discussing the beauty and, more important to him, the utility of mathematics. The bulk of his speech was a survey of the history and progress of mathematics. Geometry had remained in much the same state as it was left by the Ancients, Euclid, Archimedes and Diophantus. However, algebra was a modern subject in which Viète, especially, had made great progress. Anderson, David's great-grandfather's cousin, was also mentioned briefly here. Napier had given the world logarithms, and Cavalieri his methods of indivisibles. This last science, clear enough to a geometer, was nevertheless attacked by some philosophers, more peripatetics than geometers, but it had been wonderfully defended by Torricelli, Wallis and Barrow. Gregory praised Descartes extravagantly, along with Hudde, Sluse and de Witt. Descartes had set the doctrines of analytical geometry on a clear and firm basis, and Fermat had also worked on this.

Meanwhile, Torricelli and more especially John Wallis had set the study of infinite figures on a secure basis. Wren and others had shown that Descartes was wrong in denying the possibility of rectify-

⁴⁶ David Laing A Catalogue of the Graduates of the University of Edinburgh (Edinburgh, 1858) 123.

⁴⁷ S.U.L. MS QA33 G8D1.

ing curves. Finally, James Gregorie and Mercator had shown the power of infinite series, and the work of James Gregorie at the end of his life contained many new results based on them. David declined to enumerate the many problems which were still left to solve.

This speech presents no surprises, but is the account we would expect from a well-informed Scot of the progress of mathematics in 1683. He restricts himself to what we would call 'pure' mathematics, with no discussion of recent progress in fields such as astronomy and optics, although he mentions the usefulness of mathematics in such areas. We might have expected, though, that he would have mentioned Newton's work, on infinite series. Even if he did not hear it discussed in London in 1681, he must have known something of it from the letters John Collins had written to James Gregorie. David's knowledge of Newton's work at this time is discussed in Chapter 3.3.

Gregory was at once involved in his lecture course, which covered a wide range of topics; optics, mechanics, astronomy, geometry, trigonometry and logarithms. These are examined in Chapter 2. His first year was also busy with the preparation and publication of his Exercitatio Geometrica, in which he produced his version of the methods James Gregorie had employed to find the results he sent to Collins. This work is the subject of Chapter 3. Following the Exercitatio, Gregory set to work on a 'second method of quadrature', in which he was encouraged by Pitcairne. This work was probably inspired by John Craige, a young Scotsman who met Pitcairne and Gregory in Edinburgh in 1685. Craige believed that this method had been devised from what he had imparted to the two friends of Newton's similar work. The method and Craige's accusations are also discussed in

Chapter 3.

Gregory's duties as Edinburgh University's professor of mathematics are outlined in Chapter 2, but we have very few details of his private life there. The libels against him which were laid before the committee of visitation picture him as a drunken, lecherous, pugnacious atheist, drinking with prisoners in the Canongate gaol and there plotting the overthrow of the Government - but spending little of his time teaching and even less in Church! However, the libels were unproven and, had this been his true character, the anonymous libeller would not have been the only one to point it out. Craige, Hearne and Flamsteed all criticised him strongly - but none pointed to traits such as these.

Instead, we know of only one episode concerning Gregory's private life at Edinburgh. On 17th March, 1687 the Lords of Session heard the case of Captain Scott of the King's Life Guard, who

'having lost his dog in the College of Edinburgh, beats Mr Gregory Professor of Mathematics, by mistake, thinking he had taken his dog!'⁴⁸

The University saw this assault as an affront to their dignity, and complained in a body, whereupon Captain Scott was 'put to crave pardon'⁴⁹. This intriguing case unfortunately yields no further details; were Scott and Gregory old enemies, or did Gregory look like a man who stole other men's dogs? Was there a general animosity

⁴⁸ Decisions of the Lords of Session ed. Sir John Lauder, Lord Fountainhall (Edinburgh, 1759-61) 1 452.

⁴⁹ Ibid.

between Life Guards and College? Had Gregory in fact taken the dog?

However, one event did occur which certainly influenced Gregory in his scientific work at least. In 1687 Newton's Principia was published.

1.5 The 'Notae' in Newton's Principia

Through his uncle's correspondence, Gregory knew of Isaac Newton as the inventor of a reflecting telescope and as a gifted mathematician. He may have known of Newton's theories of light and colour, though they are not mentioned in his papers before he moved to Oxford. Throughout the 1680's, though, largely through John Craige, he had learnt more of Newton's mathematical skills. Probably it was also through Craige that he learnt of the forthcoming publication, for as early as October, 1686 he wrote to Colin Campbell that

'Mr Newton in Cambridge hath just now published a book of astronomie, it will containe many miscellanea'⁵⁰.

On 2nd February he mentioned Newton's method of quadrature

'which I am certainly informed will be published in that Astronomie'⁵¹.

In the early Autumn of 1687, David received his copy of the Principia. On 2nd September he wrote to Newton

my most hearty
'to give you thanks for having been at the pains to teach the world that which I never expected any man should have knowne, for such is the mighty improvement made by you in the Geometry, and so unexpectedlie successfull the application therof to the physics^R that you justlie deserve the admiration of the best Geometers

⁵⁰Gregory to Campbell: 2.10.1686 CCC.

⁵¹Gregory to Campbell: 2.2.1687 CCC.

and Naturalists, in this and all succeeding ages'⁵².

In that month too, David began his Notae in Isaac Newtoni Principia. These detailed comments and remarks are in the style of Schooten's notes on Descartes Geometria and are designed primarily to explain difficult points in the text, for Gregory had found many of these. To Newton he had written

'tho' your book is of so transcendent fineness and use that few will understand it, yet this will not, I hope, hinder you from discovering more hereafter to those few who cannot but be infinitely thankful to you on that account'⁵³.

He forewarned Campbell of both the interest and the difficulty of the work, saying

'I believe Newton will take you up the first month you have him'⁵⁴.

Probably the Notae began as Gregory's attempt to elucidate Newton's text for himself. We know that he made similar notes on Huygen's Horologium Oscillatorium, for in 1696 he referred to his universal theorem on the forces on weights on inclined planes which was found among these notes⁵⁵. The notes on Huygens' are now lost, and

⁵² David Gregory to Isaac Newton: 2.9.1687 NC II 311 484.

⁵³ Ibid.

⁵⁴ Gregory to Campbell: 16.13.1687 CCC.

⁵⁵ C20

we have little knowledge of their scope and contents, but Gregory apparently had no desire to publish these as he did his Notae. They were notes for his own convenience on the work itself and anything arising out of it. The Notae were probably intended at first to fill a similar role, and only later did the idea of publication arise, perhaps only in the second stage of composition.

By April, 1688, Gregory had reached corollary 1 to proposition 44 in section 9 of book 1 (page 30). The next date occurs 4 pages later among the notes on proposition 49 in section 10 and is 'Oxoniae 23 Dec^r.1692' (page 34). The dates on the main text run until 'Oxonii 29 Januarii [1694]' on page 506. Later notes were inserted on a variety of dates, normally after a talk with Newton. We may therefore distinguish three stages in the Notae; the first from September, 1687 to April, 1688 comprising notes to the end of section 9, book 1 or thereabouts, the second from December, 1692 to January, 1694 during which the notes were completed and the third over the rest of Gregory's life in which additional notes were inserted. There is no apparent extra difficulty around the point Gregory had reached in April, 1688, so it was probably the pressure of other commitments which led him to halt where he did. At that time he was busy with his 'second method' and its publication (to which the omission of Newton's quadrature methods from the Principia may have prompted him). Then followed arguments with the Town Council and the visitation committee and his removal to Oxford. It was only at the end of 1692, when he was settling into his first full academic year at Oxford, that Gregory had the leisure to take up his Notae once more. **

The Notae are described by Wightman and by Cohen⁵⁶. Generally

they attempt to make plain all that is obscure in the Principia - a task whose final completion defeated even Gregory's industry and enthusiasm! However, although some points are left unexplained, the notes are very thorough and extremely detailed.

Gregory gives details of Newton's calculations and adds frequent references to other texts. His own Exercitatio or the 'second method' are often mentioned⁵⁷. He also uses the work of James Gregorie and Christian Huygens, along with many others. In a typical case, Newton, in proposition 19 of book 3, has stated that centrifugal force at the equator was to the force of gravity there as 1 to 290.8. Gregory's Notae show how to calculate these figures, but find a value of 1 to 289, as Huygens had given in his Discours sur la cause De la pesanteur. The discrepancy, Gregory explains arose because Newton had taken 'rounded off' figures at the start of his calculation⁵⁸.

Sometimes the Notae are mildly critical. Misprints are pointed out, but, more seriously, Gregory often wishes that Newton had shown how to reach a certain result and not simply given the result itself. He also wonders at certain omissions, generally where Newton has failed to mention the relevant work of another scientist.

One example of such criticism arose out of lemma 1 to book 3. Newton had given very little explanation here of his derivation of the ratio of forces acting on the earth, and concluded the discussion by saying that the precession of the equinoxes could be deduced thence,

⁵⁶ W.P.D. Wightman 'David Gregory's commentary on Newton's Principia' Nature 179 (1957) 393-94 I.B. Cohen Introduction to Newton's Principia (Cambridge, 1971) 189-91.

⁵⁷ Notae 28, 64, 112, 156, 189 8c.

⁵⁸ Ibid 129.

but 'let each examine it who wishes. I study brevity'. Gregory was baffled by the problem; an attempt at its solution among his papers ends in despair

'Doubtless a briefer method of investigating this is known to Newton'⁵⁹.

In his Notae to this lemma, Gregory remarked petulantly on Newton's 'I study brevity',

'I labour to be brief, I become obscure. He could at least have indicated the method by which the ratios of the sums of the forces could be investigated'⁶⁰.

Eventually, Newton did explain the matter to him, and Gregory's copy of his explanation is added to the end of his original copy of the Notae.

With some other problems, though, Gregory never discovered the full answer. Corollary 2 to proposition 91, book 1 discovered the ratio of the attraction of a sphere to that of a spheroid, and involved the integration of the square root of a trinomial. Gregory made several attempts on this problem⁶¹. He could only deduce that the quadrature of such an integrand could not be found by his method. Perhaps it was possible by Newton's? He knew by late summer of 1694 that it depended on Newton's table of integrals⁶², but he was never

⁵⁹ C61.

⁶⁰ Notae, 170.

⁶¹ C181, C46, C60, C63.

⁶² ca. July, 1694 C42 NC III 461 384-6.

able to put any explanation into his Notae, although a space was purposely left blank to receive it.

However, points such as this where Gregory, with the help he received from Newton, was unable to resolve the problems of the Principia were few. Additions prompted by Newton were made from 1694 until the latest was recorded on 21st July, 1708, only a few months before Gregory's death⁶³. Nor did these concern only the points which he had found difficult. Some months later, he noted that in May, 1694, he had been able to copy into his notes or into his own copy of the Principia almost all those things which Newton had then altered in his own copy⁶⁴. (Unfortunately, Gregory's copy of the Principia has never been found). Many such emendations were inserted into the Notae. Also Gregory included any information he had gleaned from Newton which seemed relevant. Thus, his comments on proposition 4 of book 1 discuss Hooke's claims to priority in finding the inverse square law of gravitation. A note to the scholium to proposition 35, book 3 gives an opinion of Newton's given to him in London in 1698, on the origin of the quarrels between Flamsteed and Halley. Many similar items of gossip, generally direct from Newton, are found among the additions to the Notae. The information which Gregory noted here about Newton's proposed alterations to the Principia has been collected by Koyré and Cohen and is used in Appendix IV of their variorum edition of the

⁶³ Notae 171.

⁶⁴ ca. July, 1694 C42 NC III 461 384-6.

Principia⁶⁵.

Gregory wished to publish his Notae, but this plan never came to fruition, for reasons which are discussed in Chapter 5.1.1. Several manuscript copies were made, however, and one was used later in Horsley's edition of Newton's works⁶⁶. After Gregory's death there were two plans to publish. Nicolas Saunderson wrote to William Jones on 4th February, 1713, from Christ's College, telling him that 'we have proposals here' for Gregory's Notae and asking Jones' opinion of the work which neither he nor any he knew had read. He asked especially

'...what assistance Dr Gregory has had, because it may be questioned whether Dr Gregory (though no inconsiderable mathematician) was equal to a work of ^{this kind} ~~that sort~~'⁶⁷.

Perhaps the haphazard assistance Newton had given was not felt to be sufficient; more likely the extensive alterations necessary to bring

⁶⁵ Isaac Newton's Philosophiae Naturalis Principia Mathematica The third edition (1726) with variant readings. Assembled and edited by Alexandre Koyré and I. Bernard Cohen with the assistance of Anne Whitman 2 vols, (Cambridge, 1972) ii 808-816 Appendix IV.

⁶⁶ RS MS 210 'Notae in Newtoni Principia Mathematica Philosophiae Naturalis'. This is Gregory's original copy, from which I have taken all page references.
Christ Church MS 131 'Notae in Newtoni Mathematica Principia Naturalis Philosophiae', Not in Gregory's hand, but contains corrections by him.
AUL MS 465 'Notae in Isaaci Newtoni Principia Philosophiae'.
EUL Dc. 4.35 'Notae in Isaaci Newtoni Principia'.
Isaaci Newtoni Opera quae exstant omnia ed. Horsley, (London, 1779) ii 51, 124, 134, 225.

⁶⁷ S.P. Rigaud (ed) The Correspondence of Scientific Men 2 vols, (Oxford, 1891) i 264.5.

the Notae into line with the second edition of the Principia were too daunting. In any case, the project was dropped.

Ten years later, David Gregory's son, also David was made Oxford's professor of Modern History, and in 1735 he was made Canon of Christ Church. Sometime between this date, and his appointment as Dean in 1756, he allowed a copy to be made of his father's Notae, but only on condition that no attempt was made to publish it. This copy is now in Aberdeen University Library, and the Canon's was probably that now in Christ Church, Oxford.

Our David's nephew had different views from his cousin on publication of the Notae. He was professor of mathematics at St Andrews from 1739 until 1765 and he hoped to publish his uncle's notes. Unfortunately

'the expense being too great for his fortune, and he too gentle a solicitor of the assistance of others, the design was dropped'⁶⁸.

Thus the Notae remained unpublished, known only to those who had seen one of the four manuscript copies. They are certainly useful in elucidating many points in the Principia, but with the appearance of a new edition, they became largely irrelevant. They were also in some ways supplanted by Gregory's own Astronomiae and the popularizations by authors such as Keill and Whiston.

Yet all this was far in the future when Gregory began to make his Notae on Newton's Principia in the autumn of 1687. Soon more immediate problems claimed his attention in the form of political

⁶⁸ Thomas Reid quoted in Charles Hutton A mathematical and philosophical dictionary. (London, 1796) 2 vols I 558.

upheavals which led to his leaving Edinburgh.

1.6 The Visitation

The troubles which led to Gregory's departure from Edinburgh did not begin with the appointment of the visitation committee in 1690, but with the change in the town council a year earlier. A series of Gregory's papers gives his version of the events of this period, beginning with those of August, 1689⁶⁹.

According to this account, Sir John Hall, new provost of Edinburgh (that is, new head of the town council), had a grudge against all the masters of the college except Andrew Massie, for only Massie had voted for him in the election. With Menzies, town treasurer, and George Clerk, college treasurer, he persuaded the council that the college did not bring in enough rent to pay the present salaries. The council decided to cut Gregory's salary, and those of Alexander Douglas, professor of Hebrew, and the librarian. James Sutherland, ex-professor of Botany, was to lose his pension altogether.

Not surprisingly, there was much complaint and several protests were made to the town council on behalf of these masters, though without persuading them to change their mind. Gregory wrote to Lord Tarbat, enclosing his letter in one for his chaplain, Mr Falconer. Tarbat, born near Kinghorn in 1630, had in his youth been a fervent Royalist, and had fought for Episcopalianism. He had become more of a politician and less of an idealist by the time Gregory applied to him, but he was a skilful diplomat and it was largely through his advice that Gregory was later able to retain his University place.

The argument resolved itself into those who believed (or said)

⁶⁹ B23, B24, B25, B26, B27.

that the college rents would not pay the present salaries and those who said they would. According to Gregory, the calculations of the former group were full of errors, and no doubt similar claims were made on the other side.

By September, Gregory heard from Falconer that Tarbat had spoken on his behalf to George Melville who had followed William and Mary to Britain and in May, 1689 had been appointed Secretary of State for Scotland. He was Presbyterian, but a moderate one, and although a man of no great talents, his appointment had been a popular compromise. By the end of the month, Gregory heard that Melville had expressly forbidden the provost to meddle with either himself or Sutherland.

As events continued, and gossip and rumour spread, Andrew Massie, who was a regent, emerged as Gregory's chief opponent within the university. The provost was said to have complained of Gregory that he

'was not religious and fanatick enough'⁷⁰.

The council were resolute in the matter of salary cuts and John Young, who had taught mathematics at the University until Gregory's appointment, took this moment to hand in a bill complaining about the manner of this appointment. Another letter was sent to Falconer for Tarbat, and rumours spread that Young was to be appointed in Gregory's place. In November, Falconer replied that Tarbat had contacted Melville about this matter of Young.

Meanwhile, charges such as drunkenness and popery were laid against Herbert Kennedy and Andrew Cunningham, two of the regents. Questions were asked around town and college about these two and Gregory, in an attempt to broaden the charges. The council passed an order forbidding Gregory to teach in the session 1689-90, but he was not told of it officially and continued to hold his classes as usual. Massie did his best to prevent his students from attending these classes by dictating throughout the time when some should have been with Gregory. Thomas Burnet, the fourth regent, had published theses at Aberdeen in 1686 wherein he argued against the Reformation. He was notorious for his Catholicism, and in January, 1690 the principal Alexander Munro, soon to be deprived himself for his Episcopalian views, forbade Burnet to teach.

A further quarrel blew up over the key to the college gardens, which the college treasurer had seized so that the masters might not profane the Sabbath by walking there. Munro was also being closely watched, but Tarbat supported Gregory, Kennedy and Cunningham.

The students were involved in this quarrel throughout. At the start, some had used what influence they had with the council on Gregory's behalf. Now in the spring of 1690 Massie forbade his students to compliment Gregory in their orations. In particular, he rebuked James Keill (brother of John, who was to be 'Newton's bulldog') for complimenting David's uncle, James Gregorie, in a speech. Massie claimed that James

'was basely ignorant, and could not survey Stirlin
Castle without ane Smith a Jesuit'⁷¹.

At that point David stopped saluting him in the street. Massie was especially put out by the laureation speeches given by Herbert Kennedy's class in June, 1690.

But now the Scottish Parliament, on 4th July, 1690, passed an 'Act for the Visitation of Universities, Colleges and Schoolls'⁷². Following the change to Hanoverian government, this act empowered 'The Duke of Hamilton, Earl of Argyle et alii' to visit all Scottish educational establishments and examine the abilities, morals, religion and politics of all their masters. They had the power to eject any who did not match up to their standards in all respects. This meant that not only must the masters be good teachers, proficient in their subjects, and men of impeccable moral conduct, but, more importantly, they must swear to the Confession of faith and to an oath of allegiance to the Hanoverian monarchy. The oaths which were administered were cast in an especially rigid form with no loopholes. They could not be interpreted as promises of passive obedience only. According to Munro, then principal of the college, they were stronger than the oaths which the clergy themselves were asked to swear⁷³.

The committee of sixteen men appointed to Edinburgh included the provost, Sir John Hall, Gilbert Rule who was to become the next principle^{al}, and Lord Raith, who did his best to help Gregory. He also counted the Earl of Lothian, the Master of Stair, Mr James Kirkton and all the laymen on the committee as his friends.

⁷¹ B23.

⁷² The Act is quoted from in Stewart op cit¹ 55-57.

⁷³ Alexander Monro Presbyterian Inquisition (London, 1691).

The University were called before the committee and then approaches were made to see who would lay charges against whom. Massie (with, as Gregory later noted 'the dull assistance of Mr Thomas Burnet'⁷⁴) undertook Gregory's, who in turn wrote charges against Massie and Burnet. Monro's account mentions that a Doctor of medicine, probably Pitcairne, also had a hand in the charges against Massie⁷⁵.

Lord Tarbat, in whose hands Gregory had placed himself, had advised him to take no oaths whatsoever, until he was secured in his place and protected against salary cuts. It was with this resolve that Gregory handed in copies of his last three years' teaching on 28th August, and heard the charges against him on the 29th. In Gregory's modest words,

'to admiration I answered ex tempore ... so the very ministers seemed to be outcountenance'⁷⁶.

On 2nd September, he gave in a written answer, and this was submitted to Sir Patrick Hume, another member of the committee.

The charges (called 'informations' by those who laid them and 'libels' by those against whom they were laid!) were comprehensive. The first three covered atheism, never taking the Holy Sacrament and profaning the Sabbath. These were followed by swearing and drunkenness in which Kennedy and Cunningham who were also Massie's opponents,

⁷⁴ B25.

⁷⁵ Monro op cit ⁷³.

⁷⁶ B26.

were also implicated. Next came superficial teaching, having women in his room at night, fighting with Kennedy and taking too long a break at Christmas. The tenth and final charge instanced an occasion some months before when he had purportedly visited a prisoner in the Canongate Tolbooth. When there, not only had he drunk to shameless excess, but had spoken freely against the government.

Gregory's answers to these charges were spirited with a degree of wry humour. He suggested that the charge of drunkenness

'has been misplaced from some other man's lybell (for this of lybelling seems now to be a trade) or that the adressing of mine hath been committed to someone who is not acquaint with me'⁷⁷.

As to the charge of challenging Kennedy to fight, he suggested instead,

'The true affair is my Lords, the lybeller considering what he deservs in his fear fancies me to be some Hector and discovers how silly, little and meanspirited a fellow he is'⁷⁸.

He argued fiercely against the anonymity of the charges, demanding at last that the lybeller should in turn be charged with bringing up these lies. The charges of course, were all denied as were those in the sheet John Young had laid, which apparently accused him of under-

⁷⁷ B25.

⁷⁸ Ibid.

hand dealings to steal Young's post. To prove the value of his teaching, Gregory pointed to the proficiency of his pupils. To rebut the charge of drunkenness, he pointed out his regard for his own health. Against the charge of atheism, he argued

'it is impossible for a reasonable thinking man to be an atheist so that to accuse me of that is to accuse me to be somewhat which it is impossible for me to be, and since the visible things of God do show the invisible God, I must tell you my Lords that I know so much of the vastness, order and harmony in the great parts of the universe, such a symmetry and convenience in the laws by which they act one upon another that I cannot but have the due notion and impressions of a God and his infinite attributes of power and providence which becomes a philosopher, a Christian'⁷⁹.

The committee were not pleased with the task before them. The Earl of Lothian, especially, by Gregory's account, tried to avoid it. Nevertheless, the charges against Monro were found proven in early September. Witnesses were cited against Gregory, Kennedy and Cunningham, while Gilbert Rule believed he had found many places in Cunningham's logic notes which tended to error. The 'errors' turned out to be Cartesian tendencies, but after some argument the committee decided that these were not necessarily errors.

Gregory and Monro were witnesses against these two regents, but

⁷⁹ Ibid.

denied the charges, as did almost all of the other witnesses. The witnesses against Gregory, who included John Keill, denied everything. Nonsensical questions were often asked; 14 year olds, for example, were asked if they saw Cunningham commit adultery eight years ago, and the students were asked whether their masters took them into 'bawdy houses' and taverns.

Charges were now also laid against Massie. These included his methods of 'poaching' students from the other regents, negligent teaching with poor attendance and owing the library £20 since 1680. He was said to have sworn all Presbyterian oaths and to have been brought up in that faith, but then to have also sworn all the oaths in King Charles' reign. Gregory's account also listed the obligatory charges of atheism, drinking and swearing along with 'unnatural disowning his daughter' and cheating his sister. These last were not found to be relevant.

The committee examined the lecture notes and library books, disapproving of the Dauphin's Livy and Tacitus, but finding Calderwood's History of the Kirk of Scotland 'a book indeed for a bibliothèque'. (Calderwood was a fervent Presbyterian who had been given financial help by the Kirk to enable him to finish his history.) Gilbert Rule reported on Gregory's teaching methods and dictates, finding them perfectly satisfactory.

Meanwhile, the oaths were being put to the masters. Massie professed his willingness to swear, but Burnet and Cunningham refused to do so until cleared of the charges against them.

Kennedy

'made a fashion of doing [as Burnet and Cunningham had], but withall said he heartily wished the government might never change'⁸⁰.

Gregory was asked next and he too, refused to swear until cleared of the charges, as Tarbat had advised him. He held to this argument throughout the visitation proceedings.

The St Andrews visitation committee on 24th September, put out all the masters except Menzies, Mulliken and David's brother James Gregorie, on a charge of disaffection with the government and refusal to take the oaths. Apparently James had not followed his brother's example in this matter. However, on the 27th these three masters were also put out on a charge of contumacy. Glasgow had been visited on the 26th, and three masters put out.

On the 24th, at Edinburgh, Principal Monro and then Strachan, the professor of Divinity, were dismissed. Burnet and Drummond, the professor of Humanity, refused to take the oaths and were also put out. Massie swore the oath, though, and, in spite of their earlier stand, Kennedy and Cunningham also took it. Douglas, professor of Hebrew, was prepared to swear to the Confession of Faith as a peace bond but not to swear to each proposition in it. Likewise, he would submit to Kirk government if this meant simply not to oppose it. This was allowed to pass for the moment, but some careless talk that evening, wherein he compared the de facto rights of William and Mary to those of Cromwell, cost him his place, and he was dismissed on the 27th.

⁸⁰ Ibid.

For Gregory, this day the 25th September, marked the crucial point in his dealings with the committee. He dined with Pitcairne and went as far as to draw up a speech in which he refused the Confession of Faith as contrary to the reformed Churches, especially to that of England. Had he given such a speech he would certainly have lost his professorship. However, before he went into the commission that evening, Lord Raith led him aside and asked if he would not follow Cunningham's example and take the oaths. Gregory simply repeated his original argument, that his salary and his position must be regularized before he did so. Lord Raith tried to persuade him, but Gregory answered that

'I designed not to be first enjoyed and then kicked',

and finally that

'if I got not that night to think on it I would infallibly misbehave, and that he would get no honour of me'⁸¹.

Raith then allowed him to absent himself, which he swiftly did with Pitcairne. That evening he again met Raith and used his father as an excuse for prevarication, asking time to settle matters with him.

Gregory turned again to Tarbat, and heard the next day that instructions had come to the committee to trouble him no further. The charges against him, if not dismissed were officially found not proven. Some pressure was still put on him to comply, but, with the connivance of the Earl of Crawford who had now joined the committee,

⁸¹ Ibid.

Gregory's case was not raised again. Tarbat told him he was

'too honest a man to be prostitute to such people'⁸²,

and continued to press Raith on his behalf. The laymen on the committee still stood his friends and Kirkton spoke privately in his defence. New regents and professors were appointed and Gilbert Rule made principal. Gregory's position was simply passed over.

In December, Gregory began teaching again without any hindrance. However, all was not quite settled. John Young was trying to create trouble again, and Lord Raith was now very angry with Gregory, probably because he now realised that he had had no intention of taking the oaths. But with Tarbat's help, Gregory visited Raith and persuaded him that indeed only the irregularity of his position had prevented him from swearing. Thus matters settled into an uneasy truce and Gregory was still at Edinburgh in the beginning of 1691.

In March and April, 1691 there was much trouble and unrest among the students. Partly this arose out of the annual fight between the students of the third and fourth years and those of the first and second. However, this year there was a strong political note in the events. Archibald Smith made a speech against Presbyterianism and the Covenant. Prince spoke vehemently against Rule and Massie and in favour of Monro and Gregory. These two students were expelled but the troubles were not over, and rioting continued. Gregory was given safe-conduct, but the students began to harass Rule, in spite of Gregory's half-hearted attempts to prevent them.

⁸² Ibid.

Gregory's position at Edinburgh in spring, 1691 was most uncomfortable. There were still rumours that the council intended to put him out, for although the visitation committee had not dismissed him for refusing the oaths it had not confirmed his position either.

Burton's remark that

'Dr Gregorie, the only truly great man among the Episcopalian professors, was wisely spared'⁸³

gives rather a false picture. Tarbat's advice and influence had enabled him to remain at Edinburgh, but at any moment his bluff might have been called.

Moreover, he now had thoughts of marriage. On 1st March, 1690 he 'first proposed love' to Elizabeth Oliphant, the girl he later married⁸⁴. His father had signed Kinnairdie over to him in 1690, but unless he returned to live in Banff, the estate would probably not have supported a wife and family. We have no details of the transaction, but it possibly also included undertaking some financial responsibility for his younger brothers and sisters. With his Edinburgh position insecure, he could not have married.

In April, 1691, he heard of a way out. On the 22nd of this month he received a letter from a Mr William Strachan, telling him that the Savilian Chair of Astronomy at Oxford was vacant. This was the escape he had hoped for, and he wrote at once to his sponsors, Lord Tarbat and ex-Principal Monro, about the position.

⁸³ John Hill Burton quoted in Stewart op cit¹ 58.

⁸⁴ E98, Hiscock 14. David refers to Elizabeth here and elsewhere as D.O.

1.7 Becoming Savilian Professor

Gregory spent the summer and autumn of 1691 in England. He called on Newton in Cambridge, met John Flamsteed and Edmond Halley, and saw to College business in London. However, his main concern was the Savilian Chair. Newton wrote him a warm recommendation for it in which he said

prudent sober undisturbed modest & judicious & in

'I do account him ~~one of the most able and judicious~~
~~mathematicians of his age now living~~ *mathematiques a great artist...* ... He ~~is reputed~~ *respected*
 the greatest mathematician in Scotland, ~~and~~ *&* that deserv-
 edly so far as my knowledge reaches, For I ~~esteem~~ *take* him to be
 an ornament ~~to~~ *of* his country'⁸⁵.

A correspondence grew up between them over the summer. Gregory was despondent about his chances of winning the post, for which Halley and John Caswell had also applied. He wrote to Newton that he believed Caswell would be appointed⁸⁶. Caswell had been tutoring mathematics at Oxford for some years and did eventually succeed to the Savilian Chair of Astronomy on Gregory's death.

Halley might have seemed a stronger rival than Caswell. He was already known to the scientific world as an observational astronomer, and it was his efforts which had seen Newton's Principia through the press. Indeed, the Royal Society itself officially supported his application for the chair. It was his lack of orthodoxy in religion which lost him the appointment, though Armitage

⁸⁵ Newton to Charlett: 27.7.1691 NC III 366 154-55.

⁸⁶ Gregory to Newton: 27.8.1691 and 26.11.1691 NC III 370 375 165-66, 181.

follows Rigaud in arguing that he was not the sceptical atheist which Whiston later described⁸⁷. Whiston said,

'Mr Halley was so sincere in his Infidelity that he would not so much as pretend to believe the Christian Religion, tho' he thereby was likely to lose a Professorship'⁸⁸.

What ever the degree of Halley's unorthodoxy (and it was not enough to debar him from the other Savilian Chair in 1704), Gregory seems to have realised that it would prevent his appointment on this occasion.

As late as 26th November, Gregory still believed Caswell would win the Chair. He waited in London, expecting to return eventually to Edinburgh. He met Fatio de Duillier and on 11th November, made notes of their discussion, which was mostly concerned with Huygens' work⁸⁹.

By the end of December, though, Gregory knew he had been appointed to the Savilian Chair. On the 28th he met Newton in London, and must already have known, for Newton advised him then on the most suitable form of his inaugural speech⁹⁰. However, although Newton seems to have been friendly towards him, the close relationship which had been developing over the summer did not last. Newton had been very angry about the unacknowledged use Gregory had made of his work in developing his 'second method of quadrature'. This break in

⁸⁷ A. Armitage Edmond Halley (London and Edinburgh, 1966) 122-24.

⁸⁸ W. Whiston Memoirs of the life and writings of Mr William Whiston (London, 1749-50) 123.

⁸⁹ C168 RG fo 72.

⁹⁰ C85 RG fos 70, 1. NC III 381 191.

their relationship is discussed in Chapter 3.1.3.

Gregory's appointment to this post gave rise to an amusing incident, which shows how he was still regarded by some of his fellow Scots. A contemporary manuscript relates this tale.

'These and suchlike stories made the Dr [Halley] to be taken for a very free thinker, and hindered him of one of the Savile Professorships at Oxford in his competition with Dr Gregory. Upon which a Scot a stranger came several times to a Coffee House which Dr Halley used, and often asked the man after him. But the Dr not happening to come, the man enquired after his pressing business. Why Sr (says he) I would fain see the man that has less religion than Dr Gregory'⁹¹.

Before he was installed as Professor, probably as a compliment to the Chair rather than to himself, Gregory was granted the degree of M.D. For this he read three lectures on Galen on 9th-11th March, 1692, in which he discussed the eye. These are discussed in Chapter 2.6.2. The first draft of his inaugural speech was made on 5th January, 1692 and the speech was given on 21st April. It is reprinted by Lawrence and Molland, whose introduction discusses its content, especially in regard to Gregory's assessment of Wren's work⁹². Its major theme, the importance of geometry to astronomy, was a favourite one of Gregory's. He had made similar points in his

⁹¹ Bod. MS Rawlinson 4.2 quoted in E.F. McPike Correspondence and papers of Edmond Halley (London, 1937) 265.

⁹² Lawrence and Molland op cit⁵.

Edinburgh inaugural lecture and the 1690 graduation theses of one of his students had also taken this theme⁹³. The second is described by Lawrence and Molland as

'the peculiar genius of the English people in
advancing natural philosophy'⁹⁴.

The work of Ward, Wren and Newton fitted both these themes, and, in the case of the first two, complimented previous holders of the Chair. The achievements of these three formed the bulk of the speech.

Thus Gregory became established in Oxford as the University's Savilian Professor of Astronomy.

⁹³ Cl90.

⁹⁴ Lawrence and Molland op cit⁵ 147.

1.8 Life at Oxford

On 17th November, 1692, Gregory gave his first public lecture as Savilian Professor, and on the 30th he was made a member of the Royal Society⁹⁵. Apart from a trip to Flanders in 1693 and visits North to his family and on work for the Union, he spent the rest of his life in England. Most of the time was spent at Oxford, though latterly he was more and more in London. As Lawrence and Molland point out, his inaugural lecture had been, for a Scotsman, surprisingly full of praise for English mathematicians, and, in a deleted sentence, he had even asked pardon for including himself among their number⁹⁶. At this stage, too, he dropped the family's spelling 'Gregorie' for the Anglicized 'Gregory'.

Lawrence and Molland's suggestion that

'the 1690 Commissioners had disillusioned him so much
that he no longer wished to be known as Scots'⁹⁷

may have some foundation, but Gregory's admiration for England and the English was not altogether new in 1691. The report he submitted in 1687 to the committee of parliament visiting schools and colleges pointed to the English educational system, with its encouragement of mathematics, as a worthy example for Scotland⁹⁸. His respect for John Wallis and Isaac Newton was formed long before the 1690 visit-

⁹⁵ AUL MS 2206/8. Records of the Royal Society of London 2nd edition, (London, 1901) 240.

⁹⁶ Lawrence and Molland op cit⁵ 147.

⁹⁷ Ibid.

⁹⁸ C215.



ation, and to that committee he had pointed to England as the true model for a reformed Church. On the other hand, he had written to Campbell in 1686 that Wallis' Algebra

'is not ill execute passing the English humour of attributing much to their own nation'⁹⁹.

This comment seems totally opposed to the spirit of the Oxford inaugural lecture and may indicate a basic change of heart.

Whether it was due more to admiration for England, or revulsion against Presbyterian Scotland, Gregory settled happily in Oxford. He continued to take an interest in Scottish religious affairs and several Episcopalian petitions, dating from his time in Oxford, lie among his papers¹⁰⁰. Some of his papers also show an interest in Scottish political affairs, and in 1707 he studied Scots Law¹⁰¹. He also possessed two papers relating to the East India Company of Scotland; one of 1696 on a proposal to erect a Navigation school, and one of the following year relating to trade in Hamburg¹⁰². On 1st October, 1698, he was made a burgess of Aberdeen along with his brothers, James and Alexander¹⁰³. His letters to Charlett, from 1692 until his death, are full of news of Scottish affairs¹⁰⁴. **

⁹⁹ Gregory to Campbell: 25.2.1686 CCC.

¹⁰⁰ B292, misc. 53, C214.

¹⁰¹ Misc. 17, RG fo 25, misc. 19-21.

¹⁰² C127, B34.

¹⁰³ Miscellany of the New Spalding Club 2 (1908) 478.

¹⁰⁴ Bod. MS Ballard 29 fos 28-52.

Perhaps we may find a clue to Gregory's attitude to Scotland in a letter he wrote to Charlett from Edinburgh on 17th September, 1692. He has been tasting

'the best Claret that ever I drank in the isle of Brittain. The vast abundance and esteem of this with the as vast abhorrence and contempt of presbytery are the chiefe things on which I dare value this our ancient Kingdom'¹⁰⁵.

While he recognised that his own future could not lie in Scotland, and made a new one for himself in England, Gregory did not drop all connections with the country of his birth, nor despair of its future.

In Oxford, he swiftly made friends with Arthur Charlett, the master of University College. He was a man of private means and generous with them, a sociable man and a patron of learning. He maintained an extensive correspondence, and had the reputation of an incorrigible gossip. In 1683, he had travelled to Scotland where he was entertained by Sir George McKenzie of Rosehaugh, who was related to Gregory's patron, Tarbat. If they had not already met, the two men certainly had friends in common.

John Wallis was also a friend of Gregory's. He was already 75 when the Scot came to Oxford, but he helped Gregory to establish himself in the scientific world. Perhaps he encouraged Gregory to write his first paper for the Transactions on the so-called Florentine

¹⁰⁵ 17.9.1692 ibid fo 30.

problem which appeared in January, 1694 (see Chapter 4.1). Certainly he published Gregory's 'second method' of quadrature in his own Algebra in 1693 (see Chapter 3.1.3). Gregory's respect for Wallis's mathematics apparently grew to affection for the man and he helped to guard his health. Charlett, writing to Sloane in 1700, talks of Wallis taking good care to hide from Gregory and himself, as well as from his son and daughter, his intention to make a long coach-trip, knowing that if any of them found out they would prevent him¹⁰⁶. When Kneller came to Oxford to paint Wallis' portrait in 1701, it was Gregory's house in which the work was done¹⁰⁷. As Wallis' health failed, Gregory substituted for him in a lecture and devoted it to a history of Wallis' work¹⁰⁸. After his death, on 28th October, 1703, Gregory wrote his biography, with lavish praise of this work. A copy of this biography in the Bodleian library notes that it was later published in 'the Universal Historical Dictionary published by Collier'¹⁰⁹. Because of the disparity in ages, the relationship was probably not a very close one, but it seems to have included a genuine affection.

Others at Oxford were Gregory's friends. Notable were Henry Aldrich, Dean of Christ Church, and his protégé Anthony Alsop who wrote a Latin ode on Gregory's wedding. Many of the scientists Gregory knew through the Royal Society visited Oxford, and he was especially close to Fatio de Duillier and Edmond Halley, fellow enthusiasts over Newtonian science.

¹⁰⁶ Charlett to Sloane: 11.7.1700 BM Sloane 4038 fo 32.

¹⁰⁷ E103 Hiscock 11-12.

¹⁰⁸ 17.10.1703 misc. 33, 34.

¹⁰⁹ RG fo.89, Bod. MS Smith 31 58.

Fatio was a brilliant, but neurotic, young Swiss, who was very close to Newton around 1690. His relationship with Gregory apparently centred on Newton's works, and the two discussed it frequently, especially in the years between December, 1691 and May, 1694, when Gregory was cut off from Newton¹¹⁰. Possibly it was Fatio who effected the reunion between them, as is suggested in Chapter 4.2. Though Gregory and Fatio were never quite so close in the following years, nevertheless Fatio was often mentioned as one of a group in which Gregory was and his activities were mentioned in Gregory's memoranda¹¹¹.

However, around the turn of the century, Fatio became involved with a mystical sect of prophets from the Cévennes, led by one Elias Marion. These people

'ranted in the streets and conducted wild séances during which frenzied men and women prophesied the imminent coming of Judgement Day'¹¹².

Fatio became secretary to the group and took down transcripts of their prophecies. In 1707, the sect was denounced by the French Church in London, and Fatio was exposed at the pillory. Newton apparently made no effort to save him, and Fatio disappeared from his circle.

¹¹⁰ 27.12.1691, C86; 1693, C76; 31.3.1693, A37; 23.3.1694, C64; 10.4.1694, C55 RG fo 79; May, 1694 C52, RG fo 76.

¹¹¹ March, 1703 RG fo 87, partly NC IV 662 402-3; E124, 102, 147, 149, 156, 170, 183 etc, Hiscock 16, 17, 21, 23, 28, 31, 35, 39 etc..

¹¹² Frank E. Manuel A portrait of Isaac Newton (Cambridge Mass., 1968) 206.

Gregory had taken some interest in the sect. In October, 1706, Fatio had brought some of the prophets into company where Gregory was, and he noted many details of their manner of prophesying and the prophecies they had made¹¹³. The following January he noted some more of these prophecies, both those Fatio had told him and those he had heard through Newton¹¹⁴. The tone of these notes is not one of conviction, but Gregory was clearly making some attempt to record what he knew of these people. . He may have felt Fatio's enthusiasm was misguided but he had no doubt of his sincerity. **

Edmond Halley was a very different type from the introverted Fatio. He was generally regarded as sociable and extrovert, sometimes even as frivolous. He and Gregory had both applied for the Savilian Chair of Astronomy, but there seems to have been no sense of rivalry between them. Halley advised Gregory on a publication for the Transactions at a time when a successful publication would have given Gregory a large advantage in this competition¹¹⁵. Halley spent much of the following years travelling, but nevertheless, Gregory met him from time to time and heard news of his travels and researches. Halley's name appears throughout the memoranda from 1691 on. Halley was involved in advising on Gregory's Astronomiae in 1701¹¹⁶. After Wallis' death, Halley was appointed to the Savilian Chair of Geometry,

113 26.11.1706 A653 RG fo 63.

114 C211'.

115 Gregory to Newton: 10.10.1691 NC III 372 169-70.

116 A68; see Chapter 5.1.

and the two men became colleagues. In November, 1706, they undertook a joint edition of Apollonius and Serenus¹¹⁷ on which they worked amicably together until Gregory's death. Flamsteed's diary and letters frequently couple them together, and in 1703 he wrote

'They are confederates; but I believe they have no confidence in one another'¹¹⁸.

However, this is the only suggestion that there was not complete trust between them. Perhaps Halley's act in telling Hearne of the error Gregory had made in his Astronomiae did not betoken complete trust, but he had told Gregory of it first and given him a chance to rectify it (see Chapter 5.4.2). It seems most likely that Flamsteed's comment was prompted by his dislike of them both. Flamsteed's relationship with Gregory became very bitter, as is discussed below, but in the early days at Oxford it was amicable enough.

Gregory was one of a colony of Scotsmen in England, whose numbers also included George Cheyne. Gregory criticised the work of this man, who followed Pitcairne in iatromechanics and wrote on both mathematics and theology. As Pitcairne said, Gregory and Cheyne were 'not indissoluble friends, tho both are mine'¹¹⁹. John Craige, too, angry over the way he felt his confidences had been abused, was not on good terms with Gregory. However, the diplomatic court physician

¹¹⁷ El80.

¹¹⁸ Flamsteed to Thornton: 18.2.1703 in Francis Baily The Life of the Reverend John Flamsteed (London, 1835) reprinted in Holland by the Kripps reprint company, 1966, 748.

¹¹⁹ Pitcairne to Campbell: 1.10.1703 CCC.

John Arbuthnott, the young doctor, James Keill and, most importantly, Gregory's protégé and James' brother, John Keill, were all friends to Gregory.

Thomas Hearne, the Oxford chronicler, disliked this group, and Charlett 'the known patron of the Scotch Men'¹²⁰. He made many snide remarks about Gregory in his diaries; his Astronomiae was stolen from Newton, his Euclid was more truly the work of Dr Hudson. In these remarks Gregory's nationality is not long forgotten. Hearne was probably not alone in his dislike of the Scottish Group, and, as a member of it, Gregory probably encountered some animosity at Oxford.

However, Oxford life was generally peaceful. In the early summer of 1693, Gregory travelled to Holland where he stayed with Pitcairne and met many of his friends. Here at last he met Christian Huygens, and discussed many scientific topics with him.

On his return he completed his Notae and in May, 1694 was reconciled with Newton. Chapters 4 and 5 show how, over this period, Gregory's work became more and more involved with Newtonian science. Gregory made many visits to Newton over these years, first to Cambridge and then to London. It is indicative of their relationship that Newton appears never to have visited Gregory.

Most of Gregory's publications were produced in the Oxford years. His Optics came out in 1695, his Astronomiae in 1702 and the edition of Euclid in 1703. A projected grand work on the calculus was never written. His 'second method' was reprinted with Newton's in Wallis' Algebra in 1693. Eight papers appeared in the Transactions;

¹²⁰ Thomas Hearne Remarks and collections I (1705-07) 2 (1707-10) Oxford Historical Society 2 & 7 (Oxford, 1885-86) i 90.

one on the Florentine problem, two on a priority dispute involving James Gregorie, two on the catenary curve, one on descent in a cycloid, one on observations of an eclipse and one on Cassini's orbit.

In 1695, he married Elizabeth Oliphant of Langton. The Oliphants were generally a Jacobite family and the Oliphants of Langton were descended from Peter, second son of the third Lord Oliphant of Gask, who died in 1566. They were near enough to the succession that in 1748 an Oliphant of Langton could still lay claim to the title¹²¹. The marriage was apparently a happy one and three years later David's brother, James, married Elizabeth's sister Barbara. David and Elizabeth had nine children, but most of them died young.

121 E. Maxtone Graham The Oliphants of Gask, records of a Jacobite family (London, 1910) 59, 142.

1.9 Tutor to the Duke of Gloucester and Flamsteed's Animosity

In October, 1653, two boys enrolled at Marischal College, Aberdeen, who were both destined to fame in their separate spheres. One was James Gregorie, David's uncle, and the other Gilbert Burnet, later Bishop of Salisbury¹²². Burnet rose quickly to an important position among the moderates of the Church. Throughout the many intrigues of his life, his policy was almost entirely one of tolerance and moderation. He found no place at James' court, but went to William and Mary at the Hague. There he was an important factor in organizing their accession to the throne and after the revolution he was rewarded with the bishopric of Salisbury. In 1698 he was appointed tutor to young William, Duke of Gloucester, Queen Anne's son.

In 1696, Newton had moved from Cambridge to London, as Warden of the Mint and Gregory also wished a London post. Perhaps this was simply because Newton was there, or perhaps more generally because it was the home of the Royal Society and the focus of British science. He wrote to Newton on 23rd December, 1697, about

'my proposal of having an establishment in London that is consistent with what I have here'.

He had heard that the Duke of Gloucester's household was being formed, and supposed that he would need a mathematics tutor, on which appointment Newton's advice would be asked. He continued

¹²² Op cit⁸ 219.

'As this would exceedingly fitt my humour and circumstances, it is such wherein I would have all probability of success: and I hope Sir you will allow me your assistance in it as you shall find reason and occasion'¹²³.

Newton's place at the Mint had been acquired for him by Charles Montagu, later Earl of Halifax, who, although nineteen years his junior, had become friendly with Newton when he came up to Cambridge in 1678. Newton had already used his influence on behalf of Edmond Halley, who was appointed Deputy Comptroller of Chester Mint in 1696. Through Montagu, he certainly had an influence at Court which could be used on such appointments as this to the Duke of Gloucester.

Burnet, too, would have supported Gregory's application. He was renowned for his preference for Scotsmen and the help he gave them to Court positions. Moreover, as a fellow-student of David's uncle, he probably took a special interest in the nephew.

There is no official record of Gregory's appointment as mathematics tutor to the Duke of Gloucester, but if he had, as seems likely, the support of both Newton and Burnet it would have been most surprising had he not been appointed. A letter of Charlett's to Sloane on 11th July, 1700 strongly implies the matter was settled. After describing enthusiastically a scheme of Gregory's for teaching mathematics he adds

'even for his own Sake (much more for the Public Interest

¹²³ Gregory to Newton: 23.12.1697 NC IV 577 253.

and Honours of this University) I begin rather to fear than wish, his removal to y^e D. of Gloucester'¹²⁴.

It would be interesting to know what Charlett feared 'for his own Sake' in the appointment. Perhaps his health was already breaking down and Charlett feared the additional strain would prove too much, or perhaps he realised that the University would be displeased if Gregory took the post. Fortunately for his fears, however, the young Duke died before the end of the month, only a few days after his eleventh birthday, and no tutor was needed.

Not only Gregory had been interested in the post. John Flamsteed, Astronomer Royal at the Greenwich Observatory believed that when the Duke's household was first discussed he had been named as mathematics tutor. This circumstance added further fuel to the disagreement that arose between him and Gregory in the winter of 1698-99.

Hard feelings had already arisen between Flamsteed and Newton over lunar observations which the Astronomer Royal was supplying Newton with in order that he might perfect his lunar theory. Flamsteed felt Newton did not appreciate the time and effort involved in compiling these places of the moon, while Newton was impatient with delays in the arrival of the observations. Flamsteed showed his annoyance in a letter to Newton of 1695. He asked for further details of the theory which Newton was devising and had promised would be imparted to Flamsteed before anyone else. He pointed out some of the necessary calculations involved in ^{re}producing these obser-

¹²⁴ Charlett to Sloane: 11.7.1700 B.M. Sloane 4038 fo 32.

vations, of which Newton seemed to have been unaware, but the real cause of his annoyance appeared in the final paragraph;

'Onely I must ^{desire} ~~acquire~~ you to acquaint Mr Bently (whom I know not) but who I am told complains that your The 2nd Edition of your Principia will come out without the **D** because I do'nt impart my observations to you that I shall furnish you to your Satisfaction in yt particular had I heard of it from your selfe I had told you the contents of this letter some days since: & assured you the fault should not be layd to my charge'¹²⁵.

Flamsteed's later account of the events of 1694 states that even then both Halley and Gregory were making exaggerated claims of the accuracy of Newton's lunar theory¹²⁶. However, the suggestion that Flamsteed was actually delaying Newton's work by withholding observations is not found in Gregory's notes before 1698, although Flamsteed's remark about Bentley suggests that others had previously made this charge.

In June, 1698, Gregory was in London and noted Newton's information that Flamsteed had been criticising his (Newton's) theories of light and colour¹²⁷. On another occasion in 1698 he set down a more serious charge

¹²⁵ Flamsteed to Newton: 2.7.1695 NC IV 517 137-8.

¹²⁶ Bailly op cit¹¹⁸ 63.

¹²⁷ A79, RG fo 62.

'On account of Flamsteed's irascibility the theory of the Moon will not be brought to a conclusion, nor will there be any mention of Flamsteed, nevertheless he [Newton] will complete to within four minutes what he would have completed to two had Flamsteed supplied his observations'¹²⁸.

Now at least Gregory knew Newton's opinion of the matter. It was in December, 1698 that he inserted into page 202 of his Notae Newton's claim that Flamsteed's lunar tables had in fact been Edmund Halley's and perhaps these comments on the lunar observations date from the same conversation.

Meanwhile, John Wallis was preparing a third edition of his Opera and he wished to include in it the measurements he believed Flamsteed had made of the earth's parallax. The astronomer sent him a letter in English discussing his results, which Wallis, finding 'nothing of it but what is fit to be published' translated into Latin¹²⁹. He sent the first two sheets for Flamsteed to check by the hand of David Gregory who was travelling to London.

The letter unfortunately contained a paragraph to which Gregory believed Newton would take exception;

'I had become closely associated with Mr Newton at that ^{the learned} time Professor of Mathematics at the University of Cambridge, to whom I had given 150 places of the Moon, deduced from my observations, previously made, and at

¹²⁸ C62 NC IV 589 276-7, Turnbull's translation 277.

¹²⁹ Wallis to Flamsteed: 10.12.1698 NC IV 597 287-88.

the time of these observations, her places as computed from my tables, and I had promised him similar ones for the future as I obtained them, together with the elements of my calculation in due order, for the improvement of the Horroccian theory of the Moon, in which matter I hope he will have the success comparable with his expectations.¹³⁰~~129~~

Gregory wrote, not to Flamsteed, but to Wallis of the displeasure these remarks would cause Newton, apparently after discussing the paper with him. Wallis wrote to Flamsteed saying he had recieved this information from one who

'is a friend of both of you but he doth not give me his Reasons why' (the paragraph displeased Newton)¹³¹~~130~~.

Flamsteed knew that this could only mean Gregory and was understandably annoyed that the Scot had not come first to him. He wrote to Newton asking whether this request was in fact made on his behalf. When he received no answer to this letter he wrote to Wallis saying he believed Gregory had taken it upon himself to suggest that the paragraph be altered and that Newton knew nothing of the matter. However, he soon received a letter from Newton which told him that he was indeed displeased with any mention of the observations he had received and that he wished the paragraph omitted. Then Flamsteed wrote again to Wallis asking him after all 'to alter y^e Offensive Innocent Paragraph as you intimated'. He suggested that Newton's

130 In Tunrbull's translation, NC IV 295 n8.

131 Wallis to Flamsteed: 28.12.1698 NC IV 598 289.

letter had been written, only to cover up for Gregory's 'officious flattery'¹³².

This last suggestion is most unlikely to be true. Newton would have been furious if Gregory had made requests in his name without his knowledge, whether he approved of the request or not. He would neither have covered up such behaviour by acceding to a request he disapproved of, nor would he have remained on intimate terms with Gregory. Indeed, it is unlikely that even Gregory could have been sure that Newton would be displeased with 'ye Offensive Innocent Paragraph'.

However, Gregory's behaviour was far from faultless. He had shown Newton the article, or at least this paragraph, which he had been given for Flamsteed. There is also much justice in Flamsteed's complaint that Gregory should have come directly to him, instead of writing to Wallis.

Flamsteed's comments on Gregory to Wallis and Newton were extremely bitter. He told them that the Scotsman was an habitué of Hindmarsh's, a book-seller's shop in Cornhill apparently renowned as a resort of non-jurors. Till 1696 the associated firm was run by Joseph Hindmarsh, a prominent Tory and high Anglican. At this date it was taken over by H. Hindmarsh, perhaps his brother, and no doubt of similar political and religious views¹³³. Flamsteed's comments, if not actually marking Gregory as a non-juror, underlined his Tory politics and high Church religious views.

¹³² Letters of Flamsteed, Wallis and Newton, Dec. - Jan., 1698/99. NC IV 596 - 605 286-305.

¹³³ NC IV 290 n5.

Flamsteed now knew that Gregory was hoping for the post of tutor to the Duke of Gloucester. He suggested that the Scot's behaviour in this affair was designed expressly to create a rift between himself and Newton. In this way Gregory would bring himself into Newton's favour and so acquire Montague's support for his application. In Flamsteed's eyes, Gregory was scheming to acquire a place which was rightly his.

His resentment was not all against Gregory. Newton's attitude also angered him. In the letter in which he told Flamsteed of his wish that the paragraph was dropped, he said

'there may be cases wherein your friends should not be published without their leave'.

To this Flamsteed added,

'where persons think too well of themselves to acknowledge they are beholden to those who have furnished them with ye feathers they pride themselves in when they have great friends etc'¹³⁴.

From this time on there was no friendship between Gregory and Flamsteed. It was David's opinion which his brother, James Gregorie, passed on to Colin Campbell in 1699;

'Mr Flamsteed has rectified above 3,000 fixed stars: but is so perversely wicked that he will neither publish nor

¹³⁴ Newton to Flamsteed: 6.1.1699 NC IV 601 296-97.

communicat his observations'¹³⁵.

The publication of Gregory's Astronomiae in 1702, in which he criticized the very measurements of parallax which had given rise to the comments on lunar observations, added to Flamsteed's resentment. Referring to the previous episode he wrote to Caswell

'It seems very strange to me that he cannot let me forget an injury he once did in the conveyance of my letter De parallaxi orbis annui, but must refresh my memory by a worse repetition I pray God forgive him'¹³⁶.

He mentioned Halley and Gregory together on several occasions. The two of them, he claimed, exaggerated all Newton's achievements, especially in regard to his lunar theory. As for their motives, Flamsteed reports his opinions on discovering that Newton's theory differed from observed values by as much as 10',

'which I did not admire [i.e. wonder at] then at all, being very sensible that the persons who so loudly on all occasions cried up his performances in amending the lunar theory and tables, did it to oblige his friendship, who had then a great interest in a great courtier [Montagu]: and considering also that [they] were persons of very ordinary skill in that part of mathematics which was concerned with the heavens and the lunar theory.

135 James Gregorie to Campbell: 29.5.1699 CCC.

136 Flamsteed to Caswell: 30.7.1702 Baily op cit¹¹⁸ 205.

But Mr Newton was not displeased with their flattery:
nor ever (that I could hear of) endeavoured to correct
them'¹³⁷.

The subjects Flamsteed had in mind when he wrote this and similar diatribes, were generally Halley and Gregory. He pictured these two as hanging about Newton with nauseous flattery, in the sole hope that he would acquire Court preferment for them. Newton's animosity towards him, he judged, sprang from his refusal to join this court of admirers - a belief which may not have been wholly without foundation.

In 1704, Prince George of Denmark agreed to undertake the expense of publishing a catalogue of Flamsteed's observations. A Royal Society committee, consisting of Newton, Wren, Gregory, Roberts and Arbuthnot, was appointed to oversee publication and Flamsteed resented the whole arrangement which he felt took matters out of his hands and hindered more than it helped publication. Of Newton's part in the matter, Flamsteed wrote,

'I soon perceived that he designed only to hinder the work by delays, or spoil or sink it or force me to comply with his humour and flatter him, and cry him up as Dr G [regory] and Dr H [alley] did'¹³⁸.

The proceedings of this body dragged on with much bickering and disagreement. Only Arbuthnot, diplomatic as ever, appears to have

¹³⁷ Flamsteed's 'History of his own life' ibid 72-73.

¹³⁸ Ibid 77-78.

The latter

sympathised with Flamsteed's difficulties. His hostility towards Newton easily encompassed Gregory too. He was delighted to learn through his assistant that Gregory had attempted to draw up tables to transform the revolutions of a screw into degrees,

'wherein he wisely had supposed the screw everywhere equal and equable. I smiled at this and promised to send them my own tables for that purpose, and showed them their mistakes, and that there were no material errors committed. This was some small mortification to them: but they had learned not to be ashamed'¹³⁹.

This state of affairs continued until Gregory's death. The business of the offensive paragraph, followed by Gregory's Astronomiae and the star catalogue, all further aggravated by tutorship to the Duke of Gloucester, meant that no friendship^d was possible between them. In Chapter 5 I have discussed the criticism in Gregory's Astronomiae, and in Chapter 6 the justification for Flamsteed's condemnation of Gregory as a 'closet astronomer'.

However, there was another charge. Did Gregory (or Halley) hang around Newton in the hope of Court preferment? It is clear from his letter to Newton about the appointment to the Duke of Gloucester that Gregory wished a post in London. Certainly in the last years of his life he spent more and more time in the capital anyway. On 21st October, 1704 Flamsteed wrote to Sharpe

'I am told that Dr Gregory has lately been in London for

¹³⁹ Ibid 80.

some time, and intends to practise physic there. Mr Halley, his colleague, has been in London all this vacation, but designs not to reside at Oxford. Dr Wallis' son offers to give his father's house to the Professors of Mathematics, if they will constantly reside in it and the university; to make it into two tenements for them; but by what I hear, it seems they have no mind to comply with the condition; so the university will not have the honour of their company, who are angling for better preferments at court, but, being pretty well understood, I am apt to think, may fail of their expectations: their ill examples I hope will have the less effect by this unsettledness of theirs¹⁴⁰.

This account is no doubt exaggerated by Flamsteed's animosity against the two Savilian Professors. Nevertheless, by 1706, Gregory had acquired a house in London. A letter in Pitcairne's 'Specimena Praxeos' to Gregory by the physician dated 25th February, 1706, that is, in termtime, is addressed to 'The hounored Doctor Gregorie, Savilian Professor of Astronomie at his house in St John's Street in Long Ditch, Westminster'. When Gregory and his wife set out for Bath, shortly before his death, they left their children, not at Oxford, but in London. Of Gregory's children, Thomas was born in Oxford on 23rd December, 1703 as had all his elder brothers and sisters been. The three youngest children, however, born on 13th April, 1705, 7th December, 1706 and 11th January, 1708, were all born in London. It seems that at some time in or about 1704, Gregory

¹⁴⁰ Flamsteed to Sharpe: 21.10.1704 ibid 218

gave up his custom of visiting London from Oxford and began instead to visit Oxford from London. His 'court appointment' to the Scottish Mint duly followed in July, 1707, though it appears he had previously worked on calculating the Equivalent.

It is impossible to separate the amount which Gregory's allegiance to Newton owed to his science from what it may have owed to any hope of preferment. Newton himself enjoyed playing patron and placing 'his' young men in academic and administrative positions¹⁴¹. It was natural that he should do so for Gregory, and natural for Gregory to want the preferment so obtained. However Gregory's genuine admiration for Newton's work dated at least from the publication of the Principia in 1687, when he began to compose his Notae. He continued to play the role of Newtonian scientist throughout his life, although before 1696 Newton's influence at Court seemed vanishingly small. Gregory shared in his mentor's change of fortune and was no doubt delighted to do so, but Flamsteed was unjustified in suggesting this was his only interest in Newton.

¹⁴¹ Manuel op cit¹¹².

1.10 Editions of the Ancient Geometers

Edward Bernard, Gregory's predecessor in the Savilian Chair of Astronomy, had formed a plan to publish the works of the ancient mathematicians. He had travelled to Leyden to consult Oriental manuscripts there, and on his appointment to the Savilian chair in 1673 he studied the relevant manuscripts in the Bodleian and Savilian libraries. He published a catalogue of manuscripts, including those bequeathed by John Selden, and gave further details of some manuscripts in the Transactions¹⁴² but the actual publication of the ancient authors was not begun until David Gregory came to Oxford.

As early as 1694, Gregory discussed with Newton 'the projected edition of these books of Apollonius at Oxford'. Newton thought this should contain a preface on the geometry of the Ancients¹⁴³. However it was Euclid's works which appeared first.

On 16th February, 1699 an agreement was drawn up between Gregory and Hudson, the Librarian, to publish an edition of Euclid in Greek and Latin. Gregory was to undertake the

'Geometry and Reasoning and y^t the schemes be proper,
correct & of a true size for ye volume'

✱✱

while Hudson was to oversee the Greek text. Wallis and Aldrich both approved the design and the latter offered all assistance with it¹⁴⁴.

¹⁴² Edward Bernard Catalogi librorum manuscriptorum angliae et tribernae (Oxoniae, 1697).
PT 14 (September, 1684) no 163 721-25.

¹⁴³ C43 RG fo 68 NC III 444 327-30. Turnbull's translation 329.

¹⁴⁴ Charlett to Sloane: 1.3.1698, B.M. Sloane 4037 fos 215, 6.

However, this does not seem to have been the final arrangement, for Gregory wrote to Charlett on 11th August, 1700, to say

'Euclid is at last entirely agreed to. Mr Hudson and I were with the Dean of Christ Church on thursday night'¹⁴⁵. **

Gregory consulted Newton several times about this work, especially about the Data and Pappus' account of them, and about his preface to the edition¹⁴⁶. Even when the work was printing, in September, 1702, Gregory wrote to Newton about some propositions in the Data which they had discussed, and in May, 1703 he noted a query for Newton whether he should put all Pappus' comments on the Porisms into his preface¹⁴⁷. This preface was dated 10th June, 1703 and it seems Gregory had no time to discover Newton's opinion on the matter; for he omitted this description. Newton later wished he had included it with Commandini's translation, for the sake of completeness¹⁴⁸.

By 28th July, 1703, the work was finished, and Gregory could write an account of it for the Royal Society, which appeared in the Transactions for January, February, 1704¹⁴⁹. This edition included all the known works which had been attributed to Euclid, with Gregory's opinion on their authenticity. It was in both Greek and Latin, and

¹⁴⁵ Gregory to Charlett: 11.8.1700 Bod. MS Ballard 29 fo 39.

¹⁴⁶ 21.5.1701 A68² NC IV 634 354-55; 3.6.1701 RG fo 79; July, 1702 RG fo 63.

¹⁴⁷ Gregory to Newton: 30.9.1702 NC IV 651 391-92; May, 1703 A65³ RG fo 63.

¹⁴⁸ 22.10.1704 E127.

¹⁴⁹ RS Cl. P xxii (1) 63; PT 24 (February, 1704) no 289 1558-1560.

until Heiberg's definitive text appeared between 1883 and 1888, it was the only complete edition of Euclid's work.

The first Greek edition of Euclid had appeared at Basel in 1553, edited by Simon Grynaeus. Unfortunately it was compiled from the two most corrupt manuscripts¹⁵⁰. As Sir Thomas Heath pointed out, Gregory's Greek edition like most others, was based on this Basel one. For the Latin, Commandini's text was used, corrected where necessary from the annotations in Bernard's books. John Hudson, as is explained in the preface, compared the Greek and Latin texts, consulting the available manuscripts only where these differed. If these agreed with the Latin text he put them in the margin; if not he pointed them out to Gregory who decided between the various readings on the basis of their geometrical sense¹⁵¹.

This work was well received. Hutton described it in 1796 as 'a fine edition'¹⁵² and more recently Frankland remarked that Gregory's edition would be found in any library, and 'will repay examination'. He continued

'it is of noble appearance, and a lasting ornament to the University which produced it. It has its defects; it is not critical; but the attainment of perfection is not to be lightly demanded'¹⁵³.

¹⁵⁰ The thirteen books of Euclid's Elements translated from the text of Heiberg. With introduction and commentary by Sir Thomas L. Heath (Cambridge, 1926) 3 vols, edition 2 with revisions i 101.

¹⁵¹ Ibid i 10.

¹⁵² Charles Hutton op cit⁶⁸ i 448.

¹⁵³ Francis W.B. Frankland The Story of Euclid (London, 1902) 139.

Unfortunately, there was some trouble between Hudson and Gregory over this work. On 21st November, 1705, Hearne noted that Hudson, at the request of Dr Aldrich, the Dean of Christ Church, had originally agreed to be joint editor of the work¹⁵⁴. The account Hudson gave Hearne implies that all the Greek and most of the Latin was in his care, and that he put far more work into the edition than Gregory did. However, when Hudson assumed that he, too, would be included in the dedication of the work to Aldrich, Gregory said it was nothing to do with him and why should he have his name put to a work of mathematics? Wallis and Mill persuaded Hudson eventually to allow Gregory the sole honour of the work for two reasons. Firstly, so that Gregory, having children, had all the Dean's gratuity for the dedication (in the event, 20 guineas to his son) and

'Secondly that he [Hudson] might do S^r Hen Saville's Professor y^e utmost Honour, tho he was sensible Dr Gregory deserved none'¹⁵⁵.

Mill then wrote the compliments to Hudson which were inserted in the preface. Further problems arose over the distribution of free copies. Although Gregory and Hudson had agreed that they would share equally in the profit of the work, Gregory, through Charlett's influence, acquired far more copies than Hudson had. This manoeuvre, according to Hearne, was

'to show that more fully how perfect a Scotch man he

¹⁵⁴ Hearne op cit¹²⁰ 2 88-90.

¹⁵⁵ Ibid 89.

was¹⁵⁶.

The comments on Euclid's Musica had, reported Hearne, been written by John Wallis and Gregory had not made it sufficiently clear that the words were Wallis' own. The preface 'which is most of it indifferent stuff' was written by Gregory, but 'some other hand' had then corrected it 'as to y^e gross faults of it'¹⁵⁷.

Hearne's account of the disagreement is no doubt exaggerated. Certainly, even without his anti-Scots venom, the account coming from Hudson, must have been biased. However, it seems probable that there was some argument over the work.

We do not know who, if anyone, corrected the preface as Hearne suggested. It was not Newton himself, for we have seen that he later wished Gregory had included Pappus' comments in the Porisms. Perhaps John Keill had read it as he had the Astronomiae. In any case, it is clear that, as with his Astronomiae, Gregory had sought the help of his colleagues and the scientific world. He had again consulted Newton frequently, who had contributed what Dr Whiteside considers 'the one exciting passage in the preface', that on the Data¹⁵⁸. Wallis had contributed a passage on the Musica and, if we are to believe Hearne, another friend corrected the preface. Newton was similarly involved in the early stages of the next projected work; an edition of Appollonius and Serenus with excerpts from more modern authors and conics.

Bernard had found an Arabic version of Apollonius' Section

¹⁵⁶ Ibid 89-90.

¹⁵⁷ Ibid 89.

¹⁵⁸ D.T. Whiteside David Gregory DSB V 520-522.

Rationis among the Selden manuscripts and had begun to translate it into Latin. However, the manuscript was defective and he gave up the attempt. Gregory, at Aldrich's request, had made a fair copy of the part he had translated¹⁵⁹, and, using this as a key, Halley taught himself enough Arabic to translate the whole¹⁶⁰. He also restored the companion tract Sectio Spatii and published the two books in 1706¹⁶¹.

Gregory had taken an interest in the progress of this work. In March, 1705, he had written to Charlett from London, sending Halley his congratulations 'upon the conquest of the Arabick MS'¹⁶². Soon he, too was playing an active part in collecting and examining these manuscripts. In November, 1705, he entered negotiations to exchange a copy of Elphinstone's History of Scotland for a Greek manuscript of Serenus from Calais. This was successfully effected in April, 1706¹⁶³. In December, 1705, he made notes on the manuscripts of Apollonius and by April, 1706 he was noting details of the Apollonius manuscripts at Oxford¹⁶⁴. At this date he mentioned that he believed there were enough sources from which to restore Apollonius' Conics

'which work I believe Mr Halley and I shall undertake'¹⁶⁵

In September, 1706, Gregory made detailed notes on the Greek manuscripts

159 B37.

160 Armitage op cit⁸⁷ 160.

161 Apollonii Pergaei de Sectione Rationis libri duo (Oxonii, 1706).

162 Gregory to Charlett: 1.3.1705 Bod. MS Ballard 29 fo 43.

163 E151, 164. Hiscock 29, 34.

164 Misc. 59-61.

165 E170.

of Serenus¹⁶⁶ and in November an agreement was drawn up between Halley and Gregory to publish Apollonius and Serenus¹⁶⁷. Gregory undertook books 1-4 of Apollonius' Conics in Greek and Latin which would be supplied from original manuscripts and from Commandini. Halley undertook the remaining books 5-8; the first three from Arabic manuscripts, and the last, now lost, from the lemmas composed for it by Pappus. He would also supply missing pieces from Viète, Adrianus Romanus, Anderson, Snell and Fermat. Gregory would then supply Serenus' De Sectione Cylindri and De Sectione Coni in both Greek and Latin. A Greek manuscript which Aldrich had acquired from the French King's library would supply these works, and it was probably the same one which had been exchanged for Elphinstone's History.

Gregory was becoming increasingly involved with his work in overseeing the Scottish Mint and spent most of the summer and autumn of 1707 in Edinburgh. He did some more work on the projected edition but was apparently not best pleased when Aldrich proposed that its scope was extended. In January, 1708 he noted somewhat resentfully that the Dean was 'to put on me' collecting all the authors on conic sections since Apollonius.^{A52} This would involve Gregory of St Vincent, Viviani, de la Hire, de l'Hôpital, and Newton's Principia and Alegbra. Typically, he noted that he would have to consult Newton, Halley and Keill¹⁶⁸. On 30th July, he added further notes on the

¹⁶⁶ RG fo 73.

¹⁶⁷ E180.

¹⁶⁸ 7.1.1708 A52.

progress of the edition¹⁶⁹. At this stage, it was intended that all authors more modern than Serenus should be under Gregory's care.

However, Gregory died before this work was completed. His own contributions had apparently gone no further than the brief notes he had made on various Greek, Latin and Arabic manuscripts. In 1710, Halley published the eight books of Apollonius' Conics¹⁷⁰.

¹⁶⁹ Oxford, 30.7.1708 RG fo 74.

¹⁷⁰ Apollonii Pergaei Conicorum libri octo (Oxoniae, 1710).

1.11 Work for the Union

On 1st May, 1707, the Act of Union between Scotland and England came into force. The Act stated

'That from and after the Union, the coins shall be of the same standart and value throughout the United Kingdom as now in England; and the present officers of the mint continued, subject to such regulations and alterations as her Majesty, her heirs or successors, or the Parliament of Great Britain, shall think fit'¹⁷¹.

This meant that all previous issues of the Scottish Mint were to be reminted, along with any foreign coins in circulation. The silver had fallen below the standard of that of the English Mint in weight and fineness and this, too, had to be corrected. The aim was to produce coins identical with those of the London Mint, differentiated only by the Mint-mark £¹⁷².

As Master of the London Mint, Newton was closely involved in this reminting operation. On 24th June, 1707, he wrote to Godolphin that he had spoken with Gregory and one of the London clerks¹⁷³. He recommended that they be sent to Edinburgh

¹⁷¹ G.M. Trevelyan Select Documents from Queen Anne's Reign, 1702-07 (Cambridge, 1969) 239.

¹⁷² NC IV 493 n4.

¹⁷³ Draft of letter P.R.O. Mint 19. 111 181.

'to instruct their Officers and Clerk and assist them in their business'¹⁷⁴.

Accordingly on 12th July, 1707, a warrant was issued appointing David Gregory to the Mint. The warrant directed him, as

'a fit person well known in the present constitution and method of the Mint in England'¹⁷⁵.

The initial appointment was for three months at a payment of £250.

Difficulties arose in the business of reminting, and Gregory wrote to Newton asking for advice, information and equipment¹⁷⁶. In particular, the unavailability of charcoal in Edinburgh meant that the furnaces were run on coal which made it impossible to follow exactly the procedure of the London Mint. Methods had therefore to be devised to compensate for this. Under Newton's directions, Gregory made experiments on such methods, until a suitable technique was devised. ¹⁷⁷

Gregory left for Scotland on 21st July, where he arrived on the 31st and remained until 15th November. By this time, he believed, processes were in tune with those of the English Mint, and they were coining 6,000 pounds a week. At this point, as he informed Godolphin

¹⁷⁴ P.R.O. T. 1/103 no 57 NC IV 724 494-95.

¹⁷⁵ P.R.O. Mint 19 111 180. Copy AUL 2206 48 D1.

¹⁷⁶ Gregory to Newton: 12.8.1707 NC IV 727 497-98.
9.10.1707 NC IV 728 498-99.

Newton to Gregory: 1707 NC IV 731 502-03.
P.R.O. Mint 19, 111, 110, Mint 19, 1, 190, Mint 19, 111, 160, respectively.

¹⁷⁷ Gregory to Godolphin: 13.12.1707 P.R.O. T. 1/103 no 94, NC IV 732 503-04.

through Newton, he felt it was unnecessary for him to stay longer. The Lord Treasurer then released him from his post, and he was eventually paid £300 for his services¹⁷⁸.

However, it was not only as overseer of the Scottish Mint that Gregory was involved in the Act of Union. It was agreed that England should pay Scotland a lump sum, the Equivalent, on Union¹⁷⁹. This was partly to compensate share-holders who had lost in the Darien project, but mainly to offset the customs and excise duties paid by Scotland which would be appropriated to pay off the English National Debt. A figure of £398,085-10s. was arrived at by a calculation based on expected customs and excise revenue. However, it was supposed that Scottish trade, and therefore the duties paid in Scotland would increase because of the better opportunities open after Union. To allow for any such rise another element was introduced, the 'rising Equivalent'. It was thus agreed that for every £1,000 such increase in customs revenue, £792 would be payable to Scotland and for every £1,000 increase in excise revenue, £625 would be payable. Scottish revenues would be revised after seven years to discover the amount due and thereafter revised annually.

These values of the fixed and rising Equivalents were calculated by a committee of six, David Gregory, William Paterson, Sir David Nairne, the Scottish secretary depute, and three English representatives. It was possibly in connection with this work that a payment of £200 as 'accomptant for the treaty' was made to David Gregory in

¹⁷⁸ P.R.O. Out letters (North Britain) 1 319-20: 26.2.1708.

¹⁷⁹ For details of the Equivalent, see P.W.J. Riley The English Ministers and Scotland, 1707-1727 (London, 1964) 203-24.

1703¹⁸⁰.

It was decreed that the Equivalent should pay off the public debts outstanding against the Scottish treasury. This included compensation for losses in the Darien scheme and for losses on recoinage as well as for arrears of salary and similar debts. Unfortunately, the Equivalent was quite insufficient for all the claims that were made against it and much hard feeling arose. The English were prompted to look into the initial calculations and found much to criticize. On the Scottish side too, doubts were raised about the basis of these calculations. It was only some 20 years after Union, with the formation of the Bank of Scotland and the abandonment of the concept of the Equivalent that these matters began to be resolved.

Of course, not all these problems stemmed from the original calculations of the Equivalent. However, it was also true that these calculations were extremely inaccurate. Essential information was unobtainable and so the committee guessed at the amounts in approximate round figures which were frequently very far from the truth. Riley suggests that although the English commissioners (and presumably the Scots too) must certainly have suspected these figures, they allowed them to pass in their eagerness not to delay Union. Of those who calculated the Equivalent he says

'The statement [of Scottish revenue] was certainly the product of wishful thinking and ignorance on the part of the Scots responsible for its compilation. It would

¹⁸⁰ P.R.O. Out letters (North Britain) I 296.

be unjust to accuse them of anything more serious¹⁸¹.

However, this 'wishful thinking' in which Gregory was involved, was to have extremely serious consequences for Scotland in the years to come.

¹⁸¹ Riley op cit¹⁷⁹ 205-06.

1.12 Illness and Death

Even before the summer of 1707, much of which he spent in Scotland regulating the affairs of the Mint, Gregory's health was failing. According to John Urry who wrote to Campbell of Gregory's final illness, the solicitude of his friends in Scotland

'that would not let him be too studious, and retired'

led to a large improvement in his health¹⁸². These friends would have included Pitcairne who was then in Edinburgh. However, on his return to London he met with a large number of Scots who were in the English Capital to deal with the suspected Jacobite invasion. Several late nights in their company undid the improvement which Scotland had seen in his health, and he never recovered from this setback.

On 12th October, 1708, Pitcairne wrote to Colin Campbell

'Meantyme my deare doctor is, in my opinion and in his owne, dying of a palpitacion and polypus cordis. he's advysed to goe to Bath for it, a ridiculous advyce'¹⁸³.

By this date, Gregory had already died¹⁸⁴.

He had taken the 'ridiculous advyce' and travelled to Bath, but had been there less than a week when he heard that his only daughter

¹⁸² John Urry to Campbell: 20.4.1710 CCC.

¹⁸³ Pitcairne to Campbell: 12.10.1708 CCC.

¹⁸⁴ Accounts of Gregory's death are given in three published letters;
 1. John Arbuthnot to Charlett: 10.10.1708, Stewart op cit¹ 74, Irving; op cit⁷ ii 262.
 2. Smalridge to Charlett: 16.10.1708, Stewart op cit¹ 75, 76.
 3. A second latter of Arbuthnot to Charlett, at some later date, Irving ii 263.

was ill in London with smallpox. He and his wife set out at once for the Capital, though Gregory was then so weak that he could only travel in a horselitter. They never reached London. When they arrived at Maidenhead, Gregory sent to Windsor for John Arbuthnot who arrived on 10th October to find him still resolved to travel on to London, from which Arbuthnot dissuaded him. That afternoon, at about one o'clock, Gregory died of consumption in the Greyhound Inn, Maidenhead.

According to Smalridge, Gregory had always told his wife that he would die young and had tried in his last months to prepare her for his death. Urry told Campbell

'he dyed like a good Xtian, and a man that was not afraid
to dye'¹⁸⁵.

Arbuthnot commented that the manner of his death was 'as became a good and wise man'¹⁸⁶. A Mr Lesley had travelled with him from Bath and attended him. As well as Arbuthnot his wife sent for her brother Dr Oliphant and Arbuthnot asked Charlett to come after the death.

Unfortunately, the daughter had already died of smallpox, and the rest of the children lay ill with the same disease. It was partly because of Mrs Gregory's worry over her family, and partly because of the lack of embalmers in Maidenhead, that it was decided to bury Gregory in the church-yard there. On her return to Oxford, his wife had a monument erected to his memory in St Mary's church. This led to later confusion, for it gave Gregory's date of birth as

¹⁸⁵ Urry¹⁸².

¹⁸⁶ Arbuthnot¹⁸⁴ 3.

24th June, 1661, and of death as 10th October, 1710. Until 1970, when Lawrence and Molland pointed out that Gregory himself gave his date of birth as 3rd June, 1659, it had always been given as that on the monument¹⁸⁷. The date of death was given sometimes as 1710 and sometimes, correctly, as 1708 depending on the source from which it had been taken.

Smalridge and Thomas Smith both estimated that Gregory had left his family in comfortable circumstances¹⁸⁸, but it seems likely that only two survived the smallpox, in 1708. David the eldest son, was born in Oxford on 14th July, 1696, and lived until 1767. He followed his father into the academic life and, after taking Holy Orders, he became, in 1723, Oxford's first Professor of Modern History. In 1756 he became Dean of Christ Church. He married Lady Mary Grey and they had several sons who created some scandal by their wild behaviour, but left no heirs¹⁸⁹. Charles, born in London on 13th April, 1705 also survived the smallpox and in 1720 was enrolled as a Westminster scholar. He died young in 1724.

Three of the Gregory's nine children had died before their parents left London for Bath. Elizabeth, the eldest daughter, born in Oxford on 29th December, 1697 had died on 1st October, 1700. John, born in Oxford on 23rd September, 1699, died on 21st March, 1701 and Thomas, born in Oxford on 23rd December, 1703 died on 12th January, 1704. Barbara, the only other daughter, had been born in Oxford on

¹⁸⁷ Lawrence and Molland op cit⁵ 173 n7.

¹⁸⁸ Thomas Smith to Hearne; Hearne op cit¹²⁰ ii 145.

¹⁸⁹ Stewart op cit¹.

9th July, 1702, and she died in London of smallpox shortly before her father's death in Maidenhead. The Gregory^e's had three more sons; James born in Oxford on 15th April, 1701, Isaac born in London on 7th December, 1706 and Philip born in London on 11th January, 1708. These boys were alive when their parents left for Bath but we have no further records of them¹⁹⁰. Isaac was Newton's godson, and Thomas Smith believed he was still alive on 2nd November, following his father's death, for he wrote to Hearne on that date hoping Newton would look after the boy¹⁹¹. It seems highly possible though, that these three boys died of the smallpox which killed their sister.

After Gregory's death, Urry wrote to Campbell describing him as
 'a credit to our nation, and very well reputed for his
 skill in his profession as ever any man that ever was
 in Oxford'¹⁹².

Arbuthnot wrote to Charlett immediately after the death of 'our dear friend Dr Gregory' that he was 'in great grief'¹⁹³. A second letter said

'I have been extremely afflicted for the loss of our
 worthy friend Dr Gregory. I am sure you have lost a
 true and sincere friend and an agreeable companion'¹⁹⁴.

¹⁹⁰ Lawrence op cit¹ 33.

¹⁹¹ Smith to Hearne: 2.11.1708 Hearne op cit¹²⁰ ii 145.

¹⁹² Urry¹⁸².

¹⁹³ Arbuthnot¹⁸⁴.

¹⁹⁴ Arbuthnot 184².

Smalridge remarked that

'He was an affectionate Husband, a tender Father, an excellent Scholar, a man of great Experience and Prudence, of ^{good} ~~deed~~ temper, of sober and religious principles, and One whom those who had the happiness to be acquainted with Him will much miss'¹⁹⁵.

Hearne may have received the news of Gregory's death unmoved¹⁹⁶, but these friends were genuinely grieved at their loss.

Three publications followed his death. In 1734, Martyn and Eames' abridgement of the Transactions contained an except from his mechanics lectures. In 1743, an edition of Marcus Manilius' Astronomy included a tract of Gregory's on the poetical rising and setting of stars and finally, in 1745 Colin McLaurin edited a translation of his lectures on practical geometry which proved very popular. Further editions of this appeared and also of his Astronomiae and Optics. All these are listed in Appendix 1.

¹⁹⁵ Smalridge¹⁸⁴ 2.

¹⁹⁶ Hearne op cit¹²⁰ ii 140.

Chapter 1

Appendix

Short titleExercitatio

Exercitatio geometrica de dimensione figurarum, sive Specimen methodi generalis dimitiendi quasvis figuras (Edinburgh, 1684).

'second method'
Solutio

Gregory's 'second method' appeared twice:
First in Archibald Pitcairne Solutio Problematis de Inventoribus (Edinburgh, 1688).
Second in John Wallis Opera 11 (Oxford, 1693) 337-80.

Opticae

Catoptricae et dioptricae sphaericae elementa (Oxford, 1695).

... Secunda editio (Edinburgh, 1713).

Elements of catoptrics and dioptrics ... to which is added a method of finding the foci of all specula as well as lenses universally ... with an introduction showing the discoveries made by catoptrics and dioptrics, by W. Browne (London, 1715).

... Second edition, to which is added an appendix, by J.T. Desaguliers, containing an account of the reflecting telescopes ... with original letters which passed between Sir Isaac Newton and Dr J. G. relating thereunto; now first published (London, 1735).

Astronomiae
or Ast (02)

Astronomiae physicae et geometricae elementa (Oxford, 1702).

... secundaeditio revisa ... accesserunt praefatio editoris (C. Huart) cometographia Halleiana in modum appendicis ... horologium Sciotericorum tractatus, etc. 2 tom. (Geneva, 1726).

The elements of physical and geometrical astronomy ... done into English with additions and corrections (London, 1715).

Ast (26)

... second edition ... to which is annex'd Dr Halley's synopsis of the astronomy of comets. The whole newly revised and compared with the Latin, and corrected throughout by Edmund Stone, etc. 2 vol. (London, 1726).

Ast (72)

The elements of physical and geometrical astronomy ... reprint of London, 1726 edition with introduction and indexes by I. Bernard Cohen. Johnson reprint Corp. 2 vols. (New York, 1972).

Euclid

Euclidis quae supersunt omnia Ex recensione D. Gregorii (Oxford, 1703).

Euclid's elements of geometry The first six, the eleventh and twelfth books; translated into English from Dr Gregory's edition: with notes and additions ... by E. Stone (London, 1728-31).

... another edition (London, 1752).
 ?... another edition (London, 1763)?
 ... another edition (Oxford, 1802).
Euclidis elementorum libri priores XII ex Commandini
et Gregorii versionibus Latinis ... edidit Samuel
[Horsley], Episcopus Roffensis (Oxford, 1802).

An excerpt from Gregory's mechanics lectures appeared in John Eames and John Martyn Philosophical Transactions abridged VI (London, 1734). 275-76.

A tract of Gregory's 'De stellarum orti et occasu poetico' was published in M. Manilii Astronomicon ex optimis ... editionibus repraesentatum ... (J.A. Fabricius de M. Manilio) (Padua, 1743).

A treatise of practical geometry in three parts ...
translated from the Latin (of D.G.) with additions
[edited by Colin McLaurin] (Edinburgh, 1745). A 10th
 edition appeared in 1787, and at least one more in
 1796.

A manuscript copy of Gregory's brief 'Life of Wallis' has a note on it stating that it was published in 'the Universal Historical Dictionary published by Collier' [Bod. MS Smith 31 58].

- Several papers appeared in the Transactions
- | | | |
|------------------------------|----|--|
| 'Florentine 1.
problem' | 1. | 'Solutio problematis Florentini de testudine veliformi quadrebilis a Davide Gregorio, M.D. ac R.S.S. communicata' <u>18</u> no 207 (January, 1694) 25-29. |
| | 2. | 'A paper asserting some mathematical inventions to their true authors, by Dr David Gregory ...' <u>18</u> no 214 (November and December, 1694) 233-36. |
| 'Descent in 3.
a cylcoid' | 3. | 'De Ratione temporis quo grave labitur per rectam data duo puncta conjungentem, ad tempus brevissimum quo, vi gravitatis, transit ab horum uno ad alterum per arcum cycloidis' <u>19</u> no 225 (February, 1697) 424-25, (Published anonymously). |
| Catenary | 4. | 'Davidis Gregorii ... catenaria' <u>19</u> no 231 (August, 1697) 637-52.
Some copies of this were also printed as a separate pamphlet, (Oxonii, 1697).
It also appeared in the <u>Acta Eruditorum</u> (July, 1698) 305-21. |
| | 5. | 'Part of a letter from Dr David Gregory to Dr Sloane, dated Oxford, October 12, 1699, containing his observations of the Eclipse of the Sun on the 13th of September last' <u>21</u> no 256 (September, 1699) 330-01. |
| Hippocrates'6.
lunula | 6. | 'A letter of Dr Wallis to Dr Sloane concerning the quadrature of the parts of the lunula of Hippocrates Chius, performed by Mr John Perks- with the further improvements of the same, by Dr David Gegory, and Mr John Caswell' <u>21</u> no 259 (December, 1699) 411-18. |

- | | | |
|--------------------|-----|---|
| Catenary
answer | 7. | 'Responsio ad animadversiones ad Davidis Gregorii
catenariam, Act. Eruditorum Lipsiae mense Februarii
An. 1699' <u>21</u> no 259 (December, 1699) 419-26. |
| | 8. | A review of Vincentio Viviani: <u>De Locis Solidis</u>
(Florence, 1701) <u>24</u> no 291 (May and June, 1704)
1607-11 (Published anonymously). |
| Cassinian
Orbit | 9. | 'De Orbita Cassiniana. By Dr Gregory' <u>24</u> no 293
(September and October, 1704) 1704-06. |
| | 10. | 'Vindiciae Matheseos Universalis Gregoriana
contra secundos Abbatis Galloysii impetus in
Historia Acad. Scient. An. MDCCIII' <u>25</u> no 306
(October, November, December, 1706) 2336-41. |

Chapter 2

The University Professor: Teaching honest
men's bairns to glour to the Starrs¹

Most of Gregory's adult life was spent as a university professor, at Edinburgh from 1683-1691 and at Oxford from 1692-1708. Most of his working life was thus spent teaching, either through public lectures or private tutorials, and we have much evidence of how he carried out these tasks. As a teacher, both directly and through the notes of his lectures which were handed down through several generations of under-graduates, he helped to educate a significant number of Britain's students. Now we can assess the direction that influence would have taken.

I have discussed fully elsewhere the elements of Newtonianism in the lectures Gregory delivered at Edinburgh², and will only briefly touch on this topic when it is relevant to the lecture course in question. Briefly, I have found that, partly because of his utilitarian attitude to education and partly because of its difficulty, Gregory largely ignored Newton's Principia when he came to write his lectures. However, he was enthusiastic in his response to the work, and several of his students were introduced privately to Newtonian philosophy.

This chapter is divided into thirteen sections. Five introductory ones deal with the background, (statutes governing the Chairs, sources,

¹ B.23 According to Gregory's account of his treatment in 1689 by Edinburgh Town Council, Baillie Bruce complained 'that Gregorie did nothing but teach honest men's bairns to glour to the Starrs'.

² My article 'David Gregory and Newtonian Science; the Edinburgh Lectures' should appear in the November 1977 volume of the British journal for the history of science.

particular students and so on) and with Gregory's ideas on education. The remaining sections deal with the lectures themselves. The Optics lectures include a discussion of lectures given at Oxford for the degree of M.D. and of Gregory's text book on optics, published in 1695 and based on these lectures. The other subjects discussed are mechanics, astronomy, logarithms, trigonometry, practical geometry and hydrostatics, as given by David Gregory at Edinburgh although there is considerable doubt in the last case that the lectures are in fact his. Next I have examined some graduation theses given by Gregory's Edinburgh students, and finally I discuss the lectures Gregory gave at Oxford. An appendix tabulates the many sources from which this chapter has been drawn.

2.1 The Edinburgh and Oxford Professorships

2.1.1 Edinburgh

The Edinburgh Chair of mathematics was first held, from 1620, in conjunction with a regentship, by one Andrew Young. However it fell vacant in 1623 and was left so until Thomas Crauford filled it in 1640, again combining it with a post as regent. After his death in 1662 it was again left empty.

Not until 1668 did the Council regularize the position by defining officially the duties of the mathematics professor (although there was then no such person). All regents and scholars were to be present at his lectures, held publicly on Tuesdays and Fridays. The content of his course was to be Arithmetic, Geometry, Cosmography, Astronomy and Optics. This post was filled in 1674 - and now distinct from a regentship - by James Gregorie, David's uncle. His death the following year created another vacancy, and the Chair was only filled again when David was appointed to it at £1000 Scots. The statutes were apparently altered somewhat to state that his lectures should take place between 10 and 11 a.m. on Mondays and Fridays³.

The academic year began on October 1st and continued with scarcely a break until the following July. Six days a week were worked, with church twice on Sunday, and the days began at five in summer and six in winter, work continuing until evening. However, this was the timetable for students, and so also for their regents. Gregory's duties were less arduous, and he summed them up thus

³ Andrew Dalzel History of the University of Edinburgh 2 vols. (Edinburgh, 1862) ii 199, 204, 221, 324, 336-44.

'The professor of Mathematiques in reading publicly twice a Week (all the other publique professors too, that is all besides the regents read twice a week from the first of December to the last of May) explains some of the ancient Geometers, and in private gives such directions as he finds most suited to the students, and explains such difficulties as occur in ther studies⁴' 4.

Although the 1668 statutes had made attendance at these lectures compulsory for all students and regents, this was no longer so in David Gregory's time. He tells us that 'Gentlemen of estates' as well as those intending to become doctors or lawyers would normally study mathematics at University. Those intending a church career studied primarily under the theology professor, but many of them were tutors to noblemen or gentry and in this case they too would study mathematics. In other words, the vast majority of serious scholars in the University would study at least a part of the mathematics course.

Gregory quotes his salary with causalities (mainly class fees) as £150 sterling, which was considerably less than his estimate of the income of the regents who had higher causalities. However, at the time these estimates were made, he felt bitter over the question of salary and this imbalance may have been exaggerated.

His duties began and ended with teaching mathematics. All matters concerned with the conduct or welfare of the students were in the hands of the regents. When his students were harassing Andrew

⁴ Gregory to Newton 8.8.1681 NCIII 368 157-62 160.

Massie during the disturbances of spring, 1691, Gregory could reply happily

'that to keep scholars in order was not his trade! '5.

It is difficult to gauge how far his teaching duties extended beyond the public lectures, for this was a matter for his own conscience. The libel presented to the committee of visitation against him claimed that he despatched between forty and fifty students between the hours of four and five each afternoon - and these boys in different classes tackling different subjects. As a defence, Gregory pointed to the abilities of his students, and stated that in fact his private lessons lasted from two till seven each day⁶. The truth probably lay somewhere between the two.

His answers to the libels also refer to lessons he was giving to an Army officer, but we have no evidence of the extent of such non-University teaching. He was also doing some amount of government work, for a note in the Secretary's Office record for 6th July 1687 reads

'Precept of £30 sterling to Mr. Gregory, Professor of Mathematics, out of the bullion, November 1686 to May 1687 "for his paines in calculating the tables for regulating the Mint and bullion" '7.

⁵ B27

⁶ B24, B25

⁷ Abstracts of the records of the Secretary's Office XIII 1686-89, 3rd series (Edinburgh, 1932) xxxvi 6.7.1687.

It is interesting to note that Gregory's connection with the Scottish Mint was formed before (and so not through) his friendship with Newton.

In total, then, Gregory's university teaching duties involved two lectures a week and private tuition each afternoon. He was also doing some external tuition and some government work, which might broadly be termed accountancy. The statutes drawn up before David's uncle took the Chair specified arithmetic, geometry, cosmography, astronomy and optics. This was probably still in force when David was appointed; certainly his lectures show a similarly broad range of topics. As he probably taught arithmetic from a text book when he found his students deficient in it, we have no lectures on that topic. Cosmography seems to be the only one of these topics which he did not teach and he replaced it with several others.

2.1.2 Oxford

The Savilian Chairs of Astronomy and Geometry were founded by Sir Henry Savile in 1619. As professor of astronomy, Gregory's duties were similar to those he had undertaken at Edinburgh. He was required to lecture twice a week, for three-quarters of an hour and to 'be of easy access to the studious who would consult (him) on Mathematical subjects'. He was also required to make diligent observations by night and day to deposit in the archives, but Gregory appears to have made no attempt to comply with this part of the statutes!

Henry Savile had indicated the syllabus to be followed. The astronomy professor was to discuss Ptolemy's Almagest, introducing such modern authors as Copernicus and Geber when he saw fit. He must

not teach any astrology whatsoever. These lectures were to be deposited in the archives with the observations⁸.

We have no information as to the amount of time Gregory spent on private teaching at Oxford - only the indirect evidence of the increasing time spent on publications and on his work for the Mint. His 'Tract on Fluxions' (see Chapter 4.3) may have been intended for his pupils, and may be an indication of his pains to introduce them to higher mathematics. There is, however, no evidence to support the supposition that this tract was intended for his pupils. The scheme for 'collegia', which he drew up in 1700 (see 2.4), indicates an interest in the problems of his students and a readiness to work hard to help them. This scheme, though, may never have been put into operation.

His lectures certainly do not appear to have met the statutory obligations. The copies which we possess appear to make up a full set, but do not supply more than a few lectures a year. These are examined in 2.13, where some reasons for the paucity of these lectures are examined.

It seems possible that Gregory, although an able and conscientious teacher at Edinburgh, under the eye of the Town Council, fell into easier ways in the laxer environment of Oxford. However, it would be unjust to treat this as more than a suspicion. Gregory protested in a letter to Charlett that

'I shall not omitt anything that may further the
design of promoting the study of Mathematics in

⁸ C. Ward Oxford University statutes (Oxford 1845) 272-84.

~~in~~ the University. And I doubt not of success, considering your own concern, and that of others about it'⁹.

This letter was written in August 1700, and probably refers to the 'collegia' scheme over which Charlett was also most enthusiastic.

However, much more material has reached us, both primary and, to a lesser extent, secondary, on Gregory's teaching at Edinburgh than at Oxford. The imbalance between his teaching activities at the two universities was perhaps not as great as the available evidence suggests, but it seems that there was some such imbalance.

Most of this chapter is concerned with the Edinburgh lectures, although the concluding section deals with the Oxford ones. Perhaps some of the Edinburgh lectures were given again at Oxford, or perhaps further Oxford lectures will yet come to light. However, it was at Edinburgh that Gregory made a name for himself as a teacher, and this must be reflected in any discussion of his teaching.

⁹ Gregory to Charlett 11.8.1700 Bod. MS Ballard 29 fo 39.

2.2 Student notebooks and other sources

The sources from which our knowledge of Gregory's teaching is gathered are displayed in appendix one^(p 285). Generally, we have several copies in various notebooks of his Edinburgh lectures, and these notebooks are situated in Edinburgh, Aberdeen and St. Andrews University Libraries and in the Bodleian Library, Oxford. We also have Gregory's original copies of many of these courses. For the Oxford lectures, we have only Gregory's copies.

Although the numbers with which we are dealing are far too small for any definite conclusions, we may find some evidence on the popularity of a course from the number of copies of it which have reached us. Indeed, using this method, the practical geometry lectures appear most popular and it was these which McLaurin decided to publish in 1745 because of their popularity among his students. The Oxford course, on the other hand, would thus appear to have been highly unpopular, but differences in practice between the universities may have been a more important factor. We might also tentatively suggest that the final (and most advanced) part of the mechanics course and the lectures on gnomonics and on spherical trigonometry were less popular than the other courses.

It can be seen from the table that a problem arises because many of the courses are attributed both to David and to his brother James. In many cases, no lecturer is mentioned at all. Often, we have David's own copies of the lectures to assert his authorship. Otherwise, common sense indicates, for example, that part three of the mechanics lectures was surely written by the man who wrote parts one, two, four and five; that is, by David. Similarly, only one notebook suggests

that the lectures on logarithms, plane trigonometry and practical geometry were given by James. Since we know that David wrote the practical geometry, it seems reasonable to disregard this set of notes and attribute logarithms and plane trigonometry to David as the other notebooks suggest. This problem does not arise with the astronomy and optics lectures, nor does there seem to be any valid reason to dispute the assertion that the lectures on spherical trigonometry were David's.

However, with some of the lectures we cannot be certain of their authorship. The problem of the hydrostatics lectures is discussed below (2.11), and we must note that, if these lectures were James' there is no reason not to attribute the lectures on gnomonics to him also. The notes on arithmetic and algebra are contained in the notebook which suggests that James delivered David's geometry lectures at St. Andrews in 1696 (at which time ^{the latter} ~~he~~ was at Edinburgh). Thus, these lectures, too, may have been first given by David, but, if so, they were the only lectures he ever gave in English. They are, in any case, no more than an introduction to the performance of the four basic arithmetical operations, using numbers or letters, and their authorship seems relatively unimportant.

In sum then, we know that David lectured on optics, mechanics, astronomy, plane and spherical trigonometry, logarithms and practical geometry. He probably lectured on gnomonics, but we cannot be sure of the authorship of the lectures on hydrostatics or of these on arithmetic and algebra.

The lectures were probably first given at the times shown in table 2, and here we must remember that when Gregory says a lecture

course was first given in, say, 1685, he means from December 1685 to June 1686.

The notebooks in which these lectures are contained frequently bear different dates from these above, and the initial assumption is that the lectures were read on several occasions. However, on closer examination this does not appear to have been the case. Apart from minor alterations in word order which leave the sense unchanged, or very occasional explanatory additions, these lectures are the same no matter when they were given. (There is one interesting partial exception to this rule, however. Some of the astronomy notes contain a section on astrology which is not in the others).

Indeed, when we examine the content of some of the lectures, this unaltered state is even more surprising. For example, copies of the astronomy course, dated 1690 and 1693 describe a Cartesian universe of vortices, where comets pass from one to the next never to return. But David Gregory was by this time firmly convinced that the Cartesian vortex theory was untenable, and had accepted the Newtonian view of regularly returning comets. Numerous examples could also be cited of errors in the notes which are more readily explicable as copying errors than as misheard dictated notes. One notebook, even, is explicitly titled as a copy of lectures given first in 1686, yet it is dated 1710¹⁰.

Some additions do enter the texts; later copies of part four of the astronomy lectures sometimes have a table explaining the signs for the planets and the constellations of the zodiac, and the two latest copies of the practical geometry contain English notes on surveying

¹⁰ Bod. MS Savile 98

similar to those incorporated into the text by McLaurin when he published the notes in 1745. These are not, however, found in Gregory's copies of the lectures, and it seems likely that they were first introduced by someone else - either a student copying the notes or a teacher helping to explain them. Indeed, David Irving suggests that the notes on surveying may have been those of Robert Stewart, professor of natural philosophy at Edinburgh University in the early eighteenth century¹¹.

The discussion of astrology is the only suggestion that David Gregory may have given any of the lectures twice. His original copy does not contain this section, but it is in the copy at Christ Church College, Oxford which, although not in his hand has been corrected by him. This section is one of the rare occasions on which Gregory expressed pro-Jacobite sentiments, and political considerations may have prompted him, or a student copying his notes, to omit it. **

Thus it appears that Gregory's lectures were generally copied by students from earlier notebooks rather than dictated afresh. It is clear from the dates on the notebooks that not only his own students, but those under other teachers were doing so, often long after his death. It is possible that Gregory read some of the lectures more than once, or that his brother dictated them when he in turn became professor of mathematics at Edinburgh. However, there is no compelling evidence to suggest this and it seems most likely that the lectures were read once and thereafter copied by succeeding generations of undergraduates.

Christine Shepherd points out that the sale of lecture notes

¹¹ David Irving Lives of Scottish writers 2 vols. (Edinburgh 1839) ii 264.

was by no means uncommon in Scottish Universities of this period. She attributes this to 'the perennial student desire to avoid lectures' but this was probably not the whole reason¹².

The sons of McKenzie of Delvine, Alexander and the younger twins, Kenneth and Thomas, were at St. Andrews University in the second decade of the eighteenth century. The letters which they and their tutor, James Morice, wrote to their father from St. Andrews provide our fullest picture of student life at this time¹³. The boys bought dictates, or paid to have them copied, and this was a practice which had the approval of their father and of their fussy, pedagogic tutor. The latter, at least, would have been unlikely to have agreed to such a practice (and such an expenditure!) merely to save the boys the trouble of attending lectures.

Moreover, besides an anonymous course on pneumatics the lectures paid for were Scrimgeour's Logic and Gregory's Astronomy¹⁴. Alexander Scrimgeour was (or, at least, had been) a regent at St. Andrews, but of St. Salvator's, not St. Leonard's at which the boys were enrolled. Scrimgeour's Logic would certainly be extra to, rather than part of, their basic course. Alexander McKenzie, for whom Gregory's dictates were copied was attending the classes of Charles Gregory, David's younger step-brother, who had been professor of

¹² Christine Shepherd Philosophy and science in the Arts curriculum of the Scottish Universities in the 17th century (Edinburgh University Ph.D. thesis, 1975) 5-6.

¹³ W.C. Dickinson Two students at St. Andrews, 1711-16 (Edinburgh and London, 1952).

¹⁴ ibid Accounts 10.2.1715; 24.6.1713; 2, 14, 26.2.1713; 24.4.1713; 14.5.1713.

mathematics at St. Andrews since 1707. It may have been David's dictates which are referred to here, but we cannot be certain. Alexander may, in this case, have been saving trouble by having Charles' notes copied for him.

However, we know that the boys had a copy of Gregory's practical geometry, and possibly they had a fuller set. Francis Pringle, who had himself copied out Gregory's lectures as a student, was professor of Greek when the McKenzies were at St. Andrews. When Alexander arrived, Pringle wrote to his father,

'Your son has brought with him Du Pre's Horace, Pantheum, Mysticum, Euclid's Elements, Dr. Gregorie's dictates of practical geometry, Castellio's Latin Testament and an English Bible'¹⁵.

The reference to Dr. Gregorie dispels any doubt that these were the work of any Gregory other than David. Later, when the twins were leaving St. Andrews, Morice wrote to their father of books which he had in his possession and was sending to Delvine. The impression which the letter gives is that all these books were lent to Morice by Delvine and are now being returned. They include 'Gregory's manuscript course of Math.'¹⁶. This may refer to notes made by Alexander at Charles Gregory's lectures, but Alexander had left St. Andrews some years before. Also, if the notes had been lent to Morice by Delvine, it seems unlikely that they would have been the notes of the professor currently lecturing at St. Andrews.

¹⁵ Pringle to McKenzie of Delvine 28.11.1710 NLS 1423 fo 66.

¹⁶ Morice to McKenzie of Delvine 4.4.1716 Dickinson op cit¹³ (13) 66.

In both these cases, the dictates are listed among textbooks and they are clearly regarded in the same light. That is, the notes were not only copied (or caused to be copied) by lazy students in order to avoid attending lectures, but by diligent students who used them as a source of study to supplement their lecture courses.

The saving of class fees may have been an object here. At the time Kenneth and Thomas McKenzie were at St. Andrews, Charles Gregorie was charging a guinea a head. Alexander attended his courses, but the twins did not. Instead, Morice attended, and then taught the twins what he had learnt, thus saving a guinea! Later even this attendance was abandoned, and Morice taught them himself from Ozonam's Course of Mathematics. David Gregory's lectures may have been used in this way to provide a substitute for the professorial lectures. However, class fees do not seem to have been so high as to have constituted a major problem for a boy who could afford a University education at all. Dissatisfaction with the professor for other reasons may, though, have led a student to seek his mathematical education in Gregory's dictates.

Probably the lectures were used (as Alexander McKenzie used them) to supplement rather than replace professorial lectures. In any case, it is clear from the numbers which survive and from the references to them in the McKenzie letters that they formed an accepted part of an undergraduate education for many generations of students.

When the lectures were copied in this way, the student might date them with the year in which they were copied¹⁷ or the year in

¹⁷ e.g. EUL MS Dc. 6. 18

which they believed the lectures had first been given¹⁸. At the same time they might erroneously attribute them to the author by whom and the place at which they believed they were first given - as must have been the case when David's lectures were headed as those given by James at St. Andrews in 1696¹⁹.

The methods by which such lecture notes were acquired were various. No doubt poorer students would copy them for themselves. Morice bought Alexander a full copy of Scrimgeour's Logic 'of good write', and paid for Gregory's astronomy to be copied sheet by sheet. The practical geometry was sent to St. Andrews with Alexander, along with other books of his father's in 1710, but we cannot tell how McKenzie of Delvine had acquired it²⁰.

Those notebooks which we possess, and presumably very many other copies, since lost, were therefore being used as mathematical texts by generations of Scots students. If in no other way, Gregory must have influenced Scottish education. His students themselves were frequently influential men, however, and we will look at some of them in the next chapter.

¹⁸ e.g. Bod. MS Savile 98.

¹⁹ SUL MS QA35 G8L4.

²⁰ St. Andrews and Aberdeen were the traditional McKenzie Universities, but some did study at Edinburgh. It is possible, for example, that the Alexander McKenzie who graduated from Edinburgh in 1690 was John of Delvine's nephew of that name. See David Laing A catalogue of the graduates ... of the University of Edinburgh ... (Edinburgh, 1858) 138.

2.3 Gregory's students

I have divided the scholars who came under Gregory's influence at Edinburgh into three groups. First, there are those who attended the University while he was there and went on to become regents of Scottish Universities. These include Francis Pringle and James Gregorie, David's brother, but they fit more naturally into groups two and three respectively. Group two consists of those, whether obscure or important in later life that we know attended some part of his course. In the third group I have considered his brothers, James and Charles. Unfortunately we do not know enough about his Oxford students to draw any conclusions from a study of them. Others have also studied the notebooks from time to time, but their interest has been mainly historical. Unfortunately, we do not know very much about even the Edinburgh students, and the form of Gregory's influence must in the end be determined through a close study of his lectures.

2.3.1 Future Regents

First we must note that we cannot be absolutely certain that these regents were the boys of the same name who graduated at Edinburgh. While we may be reasonably sure of Gerscham Carmichael, say, I have omitted such as William Smith, regent at Aberdeen from 1693. These limitations must be borne in mind in the following discussion.

John Munro and John Craigie completed an under-graduate course at Edinburgh in 1685 and became St. Andrews regents; Munro from 1685 until at least 1696, and Craigie from 1691 until at least 1716. Gerscham Carmichael graduated from Edinburgh in 1691 and after teaching at St. Andrews became a regent at Glasgow in 1694 where he stayed until his death in 1729. John Row, who completed his course at

Edinburgh in 1692 was a St. Leonard's regent in the session 1694-95 and quite possibly for longer²¹.

Munro, Row and Craigie who all taught at St. Leonards are somewhat obscure. Munro's 1686 theses are Cartesian as we would expect from a pupil of Gregory's at that time. This approach was also, however, then typical of St. Andrews regents. We have no evidence of Row's teaching in natural philosophy. John Craigie's 1703 theses are definitely Newtonian, however, and he also discusses (and rejects) Huygens' speculations on the possibility of other inhabited worlds. In the same year his fellow regent at St. Salvator's, Thomas Forrester, was still teaching the Cartesian vortex theory.

Christine Shepherd finds that Gerscham Carmichael's dictates 'introduced more progressive ideas to Glasgow's teaching'. (John Loudon, who also came from St. Andrews, had a similar effect.) His lectures on metaphysics and ethics give ample evidence of his wide knowledge of current ideas. However natural philosophy does not appear to have been his first enthusiasm - when the regenting system was abolished at Glasgow in 1727 he chose rather to take on the Ethics class. We have little evidence, therefore, of his natural philosophy teachings, but his theses of 1707 mention gravity, light and the composition of matter and express Newtonian views on these topics. He also rejects the idea of vortices²².

²¹ ibid 127, 136, 141, 144 for details of graduation. Shepherd op cit(12) Appendix lists Scottish regents of the period.

²² Shepherd op cit(12) 263, 296, 316, 131, 134, 241, 288; James Coutts A history of the University of Glasgow (Glasgow, 1909) 196-7.

None of these men appear to have been in the forefront of the introduction of new ideas in natural philosophy. Nevertheless, their views appear to have been modern, and their adoption of Newtonianism certainly did not lag noticeably behind that of the other Scottish regents. In particular, both Row and Carmichael were teaching Newtonian philosophy in the early eighteenth century. Most importantly, Christine Shepherd suspects that Carmichael was Colin McLaurin's tutor at Glasgow²³. Perhaps, through Carmichael, Gregory's enthusiasm for Newtonian science helped to encourage McLaurin's studies in that field.

As attendance at mathematical lectures was voluntary, we cannot assert that these were students of Gregory's. However, from what evidence we have of their relatively progressive views, it seems likely that they had been exposed to these views in their student days, and David Gregory appears the natural source of such influence.

Again, although this is harder to judge, Gregory may also have influenced these regents who taught at Edinburgh at the same time as himself. Herbert Kennedy especially, regent at Edinburgh from 1687 until 1694, was a friend of Pitcairne and Gregory. The three men were frequently linked in charges laid against Gregory and Kennedy before the committee of visitation²⁴. Certainly Kennedy was one the first of the Scottish regents to be wholly converted to Newtonian

²³ Shepherd op cit(12) 131.

²⁴ R.K. Hannay The visitation of the College of Edinburgh in 1690.
Book of the Old Edinburgh Club 8 (Edinburgh, 1916) 79-100.

ideas, as presented in his 1694 theses²⁵, although even in 1689 and 1690 he had not abandoned his allegiance to Cartesian physics.

Perhaps David Gregory and his brother James, who replaced him in the Edinburgh Chair, had some influence on this conversion.

2.3.2 Some of the students

As opposed to the regents who may have been Gregory's students, we know of several who were certainly his students, but are no more than names. Among them, Charles Sinclair gave a speech in 1688 on the work of Torricelli, Boyle and Huygens. Three others, Laurence Oliphant, John Falconer and William Cooper gave speeches in 1690 which clearly showed their familiarity with Newtonian concepts (see 2.12). Unfortunately we know nothing of their future life, nor whether they were in a position to pass this knowledge on. Charles Oliphant, in an unpublished preface to Gregory's Optics²⁶ said that he had attended Gregory's lectures, but he, too, is known to us only as a physician and, probably, Gregory's brother-in-law. Perhaps Laurence was his brother.

Three students of whom we do know more are Francis Pringle, John Keill and the latter's brother James. 'The able and attractive Francis Pringle' was the son of a Border laird, who graduated from Edinburgh in 1694 and was a regent at St. Leonards from 1699 to 1747²⁷. A friend of McKenzie of Delvine, Pringle was also of Episcopalian and Jacobite sentiments. His notebook is in Edinburgh

²⁵ Shepherd op cit(12) 233.

²⁶ Cl14. (Discussed in 2.6.4).

²⁷ R.G. Cant The University of St. Andrews: a short history (Edinburgh and London, 1970) 83.

University Library, and is the most nearly complete collection we have of Gregory's lectures²⁸. Only part five of the mechanics, the spherical trigonometry and the horolographia are missing. Moreover, he apparently took some pains to complete the set, for part four of the astronomy lectures is in a different hand, and was copied for him in St. Andrews by a George Wood in 1705, whereas the other lectures are in Pringle's hand and, where dated, were copied in 1693-95.

It is hard to say to what extent Pringle may be considered a pupil of Gregory's, and how much reliance to place on the headings of the lectures. According to these headings, parts one to three of the astronomy lectures were given in Oxford in 1693, which seems highly unlikely since we have Gregory's own copies of the very different lectures he was giving at that time. It seems more likely that the heading was a guess on Pringle's part and the lectures were actually copied from a previous Edinburgh student of Gregory's. But this cannot apply to another section of the notes, the 'De ratione studii mathematici consilium' which Pringle notes as 'datum Oxonii 1695'. This outline of a suitable course of under-graduate study was not composed until December 1693 and so it could not have been in the hands of any of Gregory's Edinburgh students.

Graduating in 1694, Pringle should only have spent his first year at Edinburgh when Gregory was also there, in which case he might never have actually attended his lectures. However, his possession of this course of study suggests that he may have also studied at Oxford for some time. If not, he must have had contacts with some

²⁸ EUL Dc.6.12.

of Gregory's Oxford pupils.

Unfortunately, Pringle's appointment at St. Andrews was a professor of Greek, that is, as regent to first year students only. The students would only in their second year be attached to a regent who would instruct them in all their studies, including natural philosophy, over the following years. Pringle would have had no opportunity to teach his pupils what he had learnt from Gregory's notebooks.

In 1710, Pringle wrote to McKenzie of Delvine about his son Alexander's education and recommended that, if he wished the boy to study mathematics 'he may have a very good occasion here with Mr. Charles Gregorie'²⁹. Alexander went on to study with Gregorie and perhaps others did so on Pringle's recommendation. Clearly it was to the benefit of the University that the first-year regent appreciated the value of an education in mathematics and recommended the abilities of the present professor. He may also have suggested to his students, such as Alexander, that they follow his example in copying up Gregory's lecture notes. However, his part in spreading Gregory's teaching and the Newtonian philosophy cannot have been large.

James and John Keill were to be far more important figures. Both, particularly James, were loud in support of Gregory during the disturbances of 1690 and 1691³⁰. James did not graduate from Edinburgh, but continued his studies on the continent, concentrating

²⁹ Pringle to McKenzie of Delvine 28.11.1710 NLS M1423 fo. 66.

³⁰ B23.

on anatomy. He lectured unofficially on this study at both Oxford and Cambridge and in 1705 the latter University conferred on him the degree of M.D. (His previous M.D., from Aberdeen in 1699, implied little more than the ability to pay the purchase price!) In 1703 he settled in Northampton as a physician where he remained until his death in 1719, publishing some medical tracts and contributing two articles to the Transactions. He was a member of the iatro-mechanical school of medicine, of which Pitcairne and George Cheyne were also part. This school generally regarded his work as respectable elaborations of their theories³¹. It may be that Gregory influenced his choice of this particular brand of medicine, for Gregory, through his association with Pitcairne, was also interested in the application of mathematics to the problems of medicine. Certainly James confessed his indebtedness to his brother John on several occasions, and John had in turn been particularly influenced by David Gregory.

John Keill was Gregory's star pupil who took up his master's enthusiasms and in some ways outdid him. He graduated from Edinburgh in 1692, but then followed Gregory to Oxford where he was admitted to Balliol College. His Oxford M.A. came in 1694. He was enthusiastic about the Principia and in 1699 became deputy to Thomas Millington, Sedleian professor of natural philosophy. In 1712 he followed John Caswell, who had acquired the post on Gregory's death, as Savilian professor of astronomy, where he remained until his death in 1721. His first work An Examination of Dr. Burnet's Theory of the Earth

³¹ F.M. Valadez 'James Keill' DSB VII 274-5.

(Oxford, 1698) is described by David Kubrin as offering an alternative High Church Newtonian theology in place of the Low Church theologies of Whiston and Burnet. Most important was his Introductio ad Veram Physicam (Oxford, 1702) which was based on the course of experimental lectures on Newtonian natural philosophy which he had given at Oxford since 1694. They constitute the first such course, which attempted to prove Newtonian laws experimentally, and were very influential on later writers. He followed this with Introductio ad Veram Astronomiam (Oxford, 1718) which also presented Newtonian science at a level suitable for under-graduates. Keill became involved in the dispute with Leibniz over priority in the development of the calculus, and was the main proponent on the British side. Most importantly, though, he was an influential popularizer of the Newtonian philosophy and the first to introduce the experimental method to Oxford in this context³².

In his True Astronomy he acknowledged his debt to Gregory;

'... the late Dr. Gregory, the great Honour of our Profession and my Preceptor, whom I ought always to remember with Gratitude for it is owing to him if I have made any Advances in this Study!³³.

Keill certainly appears to have done more than Gregory to introduce Newtonian philosophy to Oxford. His works of popularization

³² David Kubrin 'John Keill' DSB VII 274-5.

³³ John Keill Introduction to the true astronomy 6th edition translation (London, 1778) xiii (first edition 1718).

were simpler than Gregory's Astronomy and more suited to the needs of under-graduates. All in all, the work of Gregory's pupil did more to broaden the appeal of Newtonianism than his own attempted.

2.3.3 David's brothers

James and Charles both followed their brother David as professors of mathematics at the Scottish Universities³⁴. James, born in 1666, was David's full-brother, while Charles, born in 1681, was a son of Gregorie of Kinnairdie by his second wife. James entered Marischal College, Aberdeen in 1680 and remained there until the session 1683-84, but his M.A. was given by Edinburgh. Laing finds that he was in Robert Lidderdale's class graduating in 1685, but he was not then entered in the Laureation book. Instead, on 24th September 1688, he graduated privately in the presence of his brother David³⁵. However, he was then already a regent of St. Andrews University, which post he filled from 1685-1691. In September 1692, he moved to Edinburgh to fill the vacant Chair of mathematics and he taught there until his retiral in 1725, when Colin McLaurin was appointed joint professor with him.

There is no doubt of his enthusiastic endorsement of the Newtonian philosophy. His graduation theses of 1690 are the first example we have of a Scottish regent whole-heartedly proclaiming the Newtonian philosophy. They list Newton's achievements and results, to which they give unstinting praise. Huygens' Traité de la Lumière

³⁴ James and Charles are C5 and C6 in Paul Lawrence The Gregory Family ... (Aberdeen University, Ph.d. thesis, 1971).

³⁵ Laing op cit(20) 127, 136.

(Leyden 1690) is also used in these theses, but in any points of contradiction Gregory takes Newton's view. Indeed, it was probably these theses which gave rise to the opinion that David was teaching the Newtonian philosophy to his Edinburgh students, a point which I discuss more fully elsewhere³⁶. This enthusiasm must have continued at Edinburgh and many students must have been infected by it.

James was only seven years younger than David. He probably attended some of his lectures at Edinburgh, and may have later taught some of them as his own. Three years after David's marriage to Elizabeth Oliphant, James married her sister, Barbara. The three letters which we have of James' written to Colin Campbell between 1699 and 1703 make it clear that the brothers were still in close touch. James gives news of David's work, especially of the progress of his Astronomiae and passes on several items of gossip which are found in very similar terms in David's memoranda³⁷.

We cannot, of course, dismiss influences which worked on both David and James - especially, of course, their father's. However, it seems highly probable that David's was the dominant influence in matters of scientific innovation. Certain similarities between James' theses of 1690 and theses written in that year by David's students (see 2.12) support this suggestion, and we may suppose that James' adoption of Newtonian philosophy was at least partly due to David.

Just as Keill was more important than David Gregory in spreading

³⁶ See n2; there is a manuscript copy of the theses in EUL MS Dc.6.12.

³⁷ CCC.

the Newtonian philosophy at Oxford, so James was probably more important in Scotland. However, the prime influence behind their work was David Gregory.

Charles, half-brother to David and James is more difficult to assess. The children of Kinnairdie by his two wives were traditionally opposed to each other in religion and politics. There was, moreover an age gap of twenty two years between David and Charles, and we have no evidence of closeness between them. All we can say is that David acknowledged his family obligations by keeping an eye on Charles' studies at Oxford.

Charles studied first at Marischal College (1696-98) and then at Glasgow (1698-99) becoming in 1699 one of the first four Snell exhibitioners to Balliol College, from where he graduated M.A. in 1704. David recorded his brother's graduation and used him as a witness when he and his wife signed a disquisition relinquishing their claims on Kinnairdie the same year³⁸. In March 1704, he recorded that his brother had left by ship for Aberdeen to see his friends in the North of Scotland³⁹. The previous December he had asked Charlett in a letter to continue his kindness to Charles⁴⁰. Although David took some interest in Charles' Oxford career, then, we have no evidence of any closeness, or any lack of it, between the brothers.

As professor of mathematics at St. Andrews which post he took

³⁸ E125 Hiscock 7, 18.

³⁹ E89 Hiscock 24.

⁴⁰ Gregory to Charlett 26.12.1704 Bod MS Ballard 29 fo 42.

up in 1707, Charles left little impression for good or ill. He did conduct the University's first experimental course in natural philosophy which was set up in 1714, but this appears to have been ordered by the Senate on the example of Glasgow. Charles was not its instigator, but only carrying out the Senate's orders⁴¹. However, this may reflect an interest acquired (from John Keill, perhaps, rather than his brother) in Charles' Oxford days. In 1739 he retired in favour of his son David.

2.3.4 Later History of the Lectures and their influence

Several scholars of the eighteenth and nineteenth centuries possessed copies of David Gregory's lecture notes, whose interest in them seems to have been primarily historical.

James Eames' knowledge of the mechanics lectures in 1734 is discussed in 2.8.5, and may come under this classification. St. Andrews University Library contains the copies of David's nephew and namesake, son of his brother Charles, who succeeded his father as professor there from 1739-65. His copy of parts one and two of the mechanics lectures was carefully correlated with the appropriate propositions of Wallis' De Motu⁴², but this is also the case with a copy of parts one to five in Christ Church College, Oxford and so may not have been the work of this nephew⁴³. It was he who also planned, as described in 1.5, to publish his uncle's Notae with the papers of his great-uncle James Gregorie.

⁴¹ Cant op cit (27) 83.

⁴² SUL QA35 G8L4.

⁴³ Ch. Ch. MS 131.

Many others have signed copies of the lectures. James Brown became professor of mathematics at Glasgow in 1796, and has signed a copy of the optics notes⁴⁴. David Laing signed another copy of these in 1816, and was a librarian of Edinburgh University Library⁴⁵. William Wallace, professor of mathematics of Edinburgh from 1819-38, signed a copy of the mechanics lectures, and those on Galen⁴⁶. Two notebooks including the lectures on optics, logarithms, trigonometry and geometry were signed in 1835 by F.P. Rigaud, the historian of science⁴⁷. The Notae, too, have been examined by several later scholars.

However, these men were not using the lectures as a source of scientific information, which was their primary function. Gregory's influence on Scottish education is not found here, but in the two directions we have examined previously; his notebooks as used in the late seventeenth and early eighteenth centuries, and in those of his pupils who continued to teach in the way he had taught them.

The following chapters examine the contents of the notebooks, which were to form a part of education in science at the Scottish Universities for several decades. This brief look at Gregory's students has shown though, that with the exceptions of his brother James, and John Keill, we have no clear evidence of the way David's teaching influenced their own. Rather, we must examine the lectures Gregory gave, and determine the sort of scientific work which his

⁴⁴ SUL QC 361 G8.

⁴⁵ EUL La 111 175.

⁴⁶ AUL 2206 6 and 2206 8 .

⁴⁷ Bod. MSS Savile 97 & 98.

students, and succeeding generations of students, were studying.

2.4 Gregory on Education

The papers from which we may discern Gregory's views on education are listed in appendix 3. The inaugural speeches both emphasise the utility, and, to a lesser extent, the beauty, of mathematics and these themes are underlined in James' speech. The recommendation to Parliament deals with education generally, and the remaining four outline specific courses of mathematical education.

The first point which emerges in all the papers is the practical, utilitarian goal of a University education. This attitude strongly influences the content of the mathematics courses. Secondly, a systematic approach to teaching mathematics is frequently emphasised and is the guideline whereby the presentation of the courses is determined. This leads to a uniform progression of topics, common to all the syllabi. As part of this systematic approach, Gregory emphasises the need for a thorough understanding, based on proving each step. It is not enough, even for the practical man, simply to know how to perform relevant calculations. He will not be able to perform them to his full advantage unless he understands why they work.

I have looked first at the attitudes which Gregory displays towards education in general, and a mathematical education in particular. Secondly, I have examined the detailed courses which he proposes, and the books he recommends. These courses will be found to relate very closely to the lectures he gave at Edinburgh.

2.4.1 General attitudes

In the inaugural speech which he gave at Edinburgh in 1683, David Gregory emphasised the utilitarian nature of his subject.

Mathematics was the supremely useful branch of knowledge. There was a delight in its study, too, but even those who were unable to recognize this must feel its usefulness. Distant voyages, the division of time, fortification, architecture and machines all depend on mathematics. Even the uncertainties of chance are here made certain. As professor he avowed his intention to explain the discipline to all men - at least, as far as was possible for their powers of comprehension! Who, he asked, is of more worth to mankind than he who strives to teach the most certain and useful knowledge of all? For such is mathematics.

It is possible that his view of education as a whole was coloured by this view of mathematics. He was concerned to see mathematics established on a firmer basis in the Universities and it was by arguing its utility that he hoped to see this happen. Mathematics was not a compulsory subject, but he hoped to see this changed. He argued so in the 1687 submission, and recommended that the professor of mathematics be involved in the final examinations of all M.A. candidates. Pointing to the example of England, he suggested that Scotland should set up more Chairs of mathematics.

Perhaps he was already convinced of the utilitarian goals of a University education, or perhaps, arguing for mathematics from its utility, he was constrained to judge other subjects by the same criterion. In either case, his 1687 submission judges all subjects by their usefulness. (Theology, scriptures, church history and such subjects are included as useful subjects by unspoken assumption).

The lower schools should thus concentrate on teaching a good accent (where example was all important) and clear handwriting. All

boys entering university should be competent Latinists, but Latin

'is not in the least necessary for Country men and tradesmen, on the Contrair it is hurtfull, since the time when they should be learning their trade is taken up by this which can never be of use!⁴⁸

Greek and Latin were essential for the study of the original authors of the sciences and scriptures. Gregory, whose own Greek, according to Hearne, was of a low standard⁴⁹ criticises the poor level of Greek and Latin in the lower schools and recommends that they therefore be taught in the Universities along with a study of the ancient authors.

The study of philosophy is that which comes under greatest criticism from Gregory's standpoint. Of this he says

'That the course of philosophy to which three years are allowed in our universities may be abridged, and since this piece of learning is that which of all others is of the least use in the aftercourse of our life; that the best years of the youth be not triffled (sic) away with it'⁵⁰.

Moreover, the regents should stick to a standard text with, at most, only a few comments of their own on it. He suggest Calbert's Philosophia Vetus et Novus.

⁴⁸ C215

⁴⁹ Thomas Hearne Remarks and collections 1 (1705-7) ed. C.E. Doble Oxford Historical Society 2 (Oxford, 1885) 89.

⁵⁰ C215.

On the other hand, professorships should be established in law and medicine on obvious utilitarian grounds. Theology should also be encouraged, and Hebrew and other 'oriental' languages taught to this end.

He ends his submission with the suggestion that comments be invited from all who have been concerned with foreign universities, but are not currently concerned in a Scottish one. Only so will reports be free from bias.

Of course, Gregory was not alone in his utilitarian stance. Christine Shepherd says that in the seventeenth century

'In Scotland the purpose of a university education was to produce educated men for the professions; ... [as in the Netherlands] learning was concerned not with finding out metaphysical truths, but rather with finding rules of action'⁵¹.

Gregory's arguments in favour of teaching more mathematics, and the criteria which he applied to the whole educational syllabus are simply an extension of this general attitude.

The view of mathematics as a utilitarian subject is another side of this attitude, and we cannot say which, if either, influenced the other. An appreciation of the utility of mathematics is found in many other aspects of Gregory's work, and it did not mean a restriction of mathematics to the more elementary studies such as arithmetic and trigonometry. The very highest branches of mathematics, although also on occasion remarkable for their beauty, were

⁵¹ Shepherd op cit (12) 337.

pre-eminently utilitarian. As Lawrence Oliphant pointed out in 1690, in a speech which Gregory had strongly influenced, Newton and Huygens had used the technique of quadrature to solve the most abstruse physical questions⁵². James Gregory's Edinburgh inaugural speech included his brother David in this context. His Exercitatio (see Chapter 3) had given the dimensions of many curves hitherto intractable, but this work was not purely theoretical. Newton himself had shown how essential the quadrature of these curves is to purely physical problems. David's own inaugural speech at Oxford, also given in 1692, has as a sub-theme the utility of geometry to problems of physics and astronomy. He makes it clear here that the advances made recently in these topics are intimately connected with, and only made possible by, advances made in higher geometry.

The courses which Gregory drew up for teaching mathematics were all based firmly on such practical aims. Secondly, Gregory emphasised in them the importance of a systematic approach to mathematics education.

Both these aspects were emphasised in the paper Gregory and Wallis drew up for Christ's Hospital. Established by Royal Charter in 1673, the Royal Mathematical School at Christ's Hospital existed to prepare boys for the Royal Navy. By the 1690's, under the mastership of Edward Pagett, who was frequently absent, the school's early promise had not been maintained. Pepys, after attending the school's examinations in 1693 determined to rectify matters, and he asked Newton to report on a new scheme for the education of Christ's

⁵² C190.

Hospital boys⁵³.

Gregory and Wallis also reported on the scheme, in a paper dated 13th June 1694⁵⁴. The report is in Gregory's hand and contains alterations by him, so it seems that it was primarily his work. It compared the old and new schemes, the latter as modified by Newton, and advised on such matters as a suitable time for the course and on public examinations.

This paper enlarged on Newton's remarks that the old scheme was too unsystematic. Anything, it said, in geometry, arithmetic, astronomy or navigation which is not built upon thoroughly known principles is 'quickly forgotten, oftentimes misapplied and never securely used'. Those men who believed they understood the rules they used, without knowing the foundations on which the rules were based, simply deluded themselves. They should not be called navigators, but their apes and imitators. The new scheme, with some amendments of Newton, Wallis and Gregory, was built on systematic principles. Like Newton's report, this pointed to the advantage such navigation schools had given to the French.

Gregory emphasized this systematic approach later in the recommendations he drew up for the lectures at Balliol College.

'But in all these things it would be a crime to propose anything without proof; for only so is the certainty and dignity of the mathematical sciences procured'⁵⁵.

⁵³ William Trollope A history of the royal foundation of Christ's Hospital (London 1834) Chapter IV. Newton's paper is NC III 452 357-66.

⁵⁴ 13.6.1694 RG fo 90.

⁵⁵ A68.

Perhaps the most interesting of all these schemes is that which Gregory drew up in 1700 as a proposal for his own teaching - the 'collegia' scheme⁵⁶. This was given to Charlett who wrote enthusiastically of it to Hans Sloane that

'Dr. Gregory has drawn a scheme w^{ch} extremely pleases me at first view, of teaching Mathematics after the manner of Foreign Colleges or Academys' 57.

He also sent a copy of it to Samuel Pepys, who replied in generally approving tones⁵⁸.

The paper proposed to set up a system of 'collegia', or what we might describe today as tutorial groups formed to study particular aspects of mathematics. These would be set up at the request of students who wished instruction in such a topic. Suitable times would be arranged, but they must meet for not less than an hour a day, three days a week. Each would last for about three months and 10 - 15 pupils seemed the most suitable number for a group.

The various topics were again to be approached systematically; each proposition explained and the whole illustrated throughout by appropriate examples, experiments and observations. Every student would be allowed to propose doubts and queries at any time. The groups would be conducted in English, with Latin phrases used only

⁵⁶ J.R. Tanner (ed) Private correspondence and miscellaneous papers of Samuel Pepys, 1679-1703 2 vols (London 1962) ii, 90-4.

⁵⁷ Charlett to Sloane 11.7.1700 BM MS Sloane 4038 fo 32.

⁵⁸ Tanner op cit (56) ii 107-11.

where there was no English equivalent. Gregory would examine weekly those students who wished to be so tested on their knowledge. Where possible they would work from a printed text, but otherwise Gregory undertook to provide suitable notes. (As the topics were generally those he had lectured on in Edinburgh, this would have provided little difficulty for him.) The courses were designed to fit the student for further studies on his own, but Gregory would always be available to any student who wanted help in this, and would advise on suitable texts.

This system, as Charlett remarked, was based on foreign Universities. However, it was not entirely new to Oxford, as Wallis pointed out in his criticisms of Lewis Maidwell's educational scheme⁵⁹. Maidwell had proposed to set up a school near London, financed by the Government, to teach forty sons of noble families. The proposal was partly intended to compensate for the fact that, unlike the French, English professors were not expected to instruct small groups.

Much of Wallis' criticism concerned Maidwell's intention to teach 'riding the great horse' or advanced equitation. On the academic side, though, he pointed out that the practice of professors taking small groups was not unknown at Oxford. Seth Ward and himself in mathematics, Staal, Plott, White and Boyle in chemistry, Musgrave, Wallis, Lower, Hannes and James Keill in anatomy and Morrison and Bobard in botany had all run such classes. Wallis presented Gregory's scheme as the latest example of a continuing tradition.

⁵⁹ T.W. Jackson 'Dr. Wallis' letter against Mr. Maidwell' Oxford Historical Society 5 1885 Collectanea 1 pt. VI 269-337.

Perhaps most revolutionary was the suggestion that classes be taken in English, long before teaching in the vernacular was accepted in the British Universities. All Gregory's own work, even that not intended for publication, was in Latin, as were his lectures, and there is no evidence that he ever did teach in English. Pepys agreed with the suggestion in principle, but continued

'yet how farr it may elsewhere be thought to affect the honour of the University yourselves are most concerned to determine'⁶⁰.

He also wondered whether it would be possible to find ten boys at the same time, with the same ability and attainments. His strongest criticism, though, was of the omission of music and perspective. These skills he felt were next in importance to Euclid's Elements for a course designed for

'the service and improvement of the youth of our nobility and gentry, the choicest and once brightest ornament of our nation'⁶¹.

That they are no longer so is generally because they are too soon exposed to 'the more gross, contagious and destructive pleasures waiting them without doors' before they have properly acquired such accomplishments as this scheme proposes. Pepys describes these as

'pleasant in the acquiring; easy in the retaining; ever usefull; ever delightfull; suited to the

⁶⁰ Tanner op cit (56)ii 108.

⁶¹ ibid ii 108.

dignity of their characters and fortunes; and
 (to crown all) lying alwayes within their own
 reach, fitted for self-entertainment and home-
 execution'⁶².

The contrast between the aims of Gregory and Pepys in education could not be more pointed, and in the place of music in the curriculum it is underlined. As for perspective, Gregory had agreed to its utility in the report for Christ's Hospital, and may well have understood it as a part of practical geometry. But he never taught music to his pupils, and never showed any wish to do so. Music may indeed produce universal pleasure, as Pepys claimed, and so help to protect our youth from the less virtuous pleasures awaiting them! However, Gregory preferred a more solid evidence of utility in his teaching. He did not teach mathematics in order to improve the character of his students, but in order to fit them with practical skills to serve whatever ends they cared to put them to.

This proposal shows Gregory at his best. It may not have been totally original, nor have encompassed the goals Pepys wished it to. Yet the scheme is practical and well thought out. Gregory's evident willingness to carry it through (although nothing of this nature is required in the Savilian statutes) shows a commitment to the task of education with which he has been entrusted. Also, it may evidence a desire to take over some of Wallis' duties, for most of the topics would be considered to fall within the scope of the professor of geometry rather than astronomy. Wallis was then an old man, and

⁶² ibid ii 108-9.

Charlett's letters to Sloane suggest a conspiracy, of which Gregory was part, to save his health despite himself⁶³. This scheme may have been partly designed to do so.

Much of the interest, though, lies in the specific topics outlined in these courses, and the recommended texts. In the next section I shall examine these details.

2.4.2 Education in Mathematics

The three papers in which Gregory set out a mathematical curriculum for under-graduates follow an almost identical pattern. Only that written in 1693 for the benefit of his students gives detailed instructions on suitable texts, but he must have considered similar sources when he planned the other two papers; one of 1697 for the use of the Balliol lectures, and one of 1700 proposing his 'collegia' scheme. This course and these sources had largely formed the basis of the lectures he had given at Edinburgh. The scheme for the Christ's Hospital boys, too, although the emphasis is rather different, is not very dissimilar to the under-graduate courses.

First, the student is supposed to be well-versed in arithmetic. This is not only a basis for mathematics, but an essential skill in all walks of life, as Gregory had already argued in his 1687 submission. As texts he suggests Wallis' arithmetical works or the practical arithmetics of Tacquet and Clavius. To the Balliol college lecturers he suggested Wingate's arithmetic. Here, as in every branch of mathematics, practice is essential.

Books 1 - 6, 11 and 12 of Euclid's Elements formed the next area

⁶³ See, for example, letter 57.

of study and the editions of Clavius or John Dee were recommended. To the Balliol college lecturers, Gregory mentioned Henry Savile's Euclid, but perhaps this was partly a tactful gesture towards the founder of his Chair. The student was warned to beware of badly arranged editions, put together in any order, which only prove confusing. The only good to be gained from these books is in returning to them later, after the Elements have been mastered from another source. The comparison will then help to give an idea of how good Euclid is.

Plane trigonometry, logarithms and practical geometry made up the next two sections and thus completed the basic groundwork. These first four sections must be studied, and thoroughly understood, before any further work is attempted. Spherical trigonometry might also be included here, but it may be postponed until it is necessary for the study of astronomy. The texts here are Theodosius' Elementa Spherica and the trigonometries of Briggs, Gellibrand, Clavius or Dechaes. For logarithms, of course, Napier's own works made the best basis, but the books of Briggs, Vlacq and others are also useful. Ozonam's tract is the best for practical geometry, but Clavius and Dechaes might also be read to advantage. Fortification or architecture might be included in this last topic, and there are many good modern texts on these subjects.

The remaining studies might be taken in any order; indeed only arithmetic and Euclid's Elements are absolutely essential to these studies. The four major branches now are algebra, mechanics, astronomy and optics with various subsidiary topics.

Algebra was concerned essentially with the resolution of equations, together with some understanding of the nature of powers and of the

arithmetic of indeterminates. Practice was strongly emphasised again here. Clavius' Algebra, Oughtred's Clavis Mathematicae and Schooten's Principia Matheseos Universalis were to be studied, and all the examples worked, especially in the last. Further examples might be found in Schooten's Exercitationes Geometricae, de Billy's works (although not his Diophantus yet), Kersey's Algebra and Schooten's De Concinnandis Demonstrationibus. Gregory suggested in the reading plan for his students that the ancient geometers might be studied at this point; Apollonius' De Sectione Coni with the notes of Eutocius and Commandini, and Serenus' De Sectione Cylindri in Commandini's edition. This might be followed by part one of Johann de Witt's Elementa Curvarum and the similar work of Gregory of St. Vincent, Vincenzo Viviani and Ozonam. Then for Diophantine algebra, the student should begin with Diophantus himself, with either Bachet's or Fermat's notes. Now the commentaries of de Billy and Kersey might be read together with as many as possible of the tracts which the English, French and Germans had written on the topic in the last thirty years. This work on Diophantus was presented in the 'collegia' scheme as a separate unit.

Mechanics was based on the principles of motion and the five simple machines with their uses in practice. It might also include hydrostatics (based preferably on a course of experiment), the laws of impact, the descent of bodies under gravity, ballistics^c, or pendula and their use in measuring time. The reading list for Gregory's students differs somewhat in that mechanics is itself included as a subsection of practical geometry. No books are recommended specifically for this study - there are many good modern authors. Instead

Gregory adds for his students the somewhat more theoretical study of geometrical physics, for which Archimedes' De Equiponderantibus, and the works of Kepler, Galileo, Torricelli and Fermat are recommended. The student should also read all relevant papers in the Transactions and the Acta. Then, if he has sufficient geometry and physics, he may progress to Newton's Principia.

Optics had made great progress in the previous century, and Gregory recommends Descartes' Dioptrics, James Gregory's Optica Promota and Barrow's Lectiones Opticae. Kepler's works, though, are not mentioned in this context; nor are Newton's papers on colour in the Transactions.

Astronomy is based on Kepler; his Epitome Astronomiae Copernicanae, Mysterium Cosmographium and Harmonice Mundi. The problems of appearances caused by the earth's daily rotation and the doctrine of spheres were well understood by the ancient authors, but Mercator's Institutiones Astronomicae gave a good introduction. Streete's Caroline Tables and Flamsteed's revisions of Hor^rox's tables should be used to study the places of the planets. Finally, Kepler's Astronomia Nova is of prime usefulness for these more advanced studies. The work here must follow a fixed pattern. First, the true, or Copernican, system is to be studied and only when it is firmly established in the mind of the student may he continue to study the apparent motion of the stars with the simplifying assumption of a geocentric universe. Planetary theory is the final stage in mastering astronomy and, if not already covered, spherical trigonometry must be studied here. There are various subsidiary subjects which may be considered here, including geography (from Varenus' Universal

Geometry), gnomonics (from Dechales or Clavius), the use of astrolabes, navigation, the calendar and the various eras and epochs. The student should now be capable of selecting books in these last topics for himself.

When the paper in which these textbooks are suggested was written, neither Gregory's Optics, nor his Astronomy had been published. No doubt he would have recommended these works as well had he written at a later date.

The three papers agree on this basic course, but we have already seen that the reading list for his students included geometrical physics, a topic not mentioned in the other papers. Two other subjects were discussed in this paper, which were not included in the others.

The first of these was Cartesian geometry. In preparation, Gregory suggests Prestet's Elemens de Mathematiques, part two of De Witt's Elementa Curvarum, de locis planis et solidis, de Graaf's Algebra and Kinkhuysen's Geometry and Algebra. Then the student might progress to Descartes' Geometry itself, as treated in Schooten's notes, and the relevant tracts of Hudde and others. (Here Gregory is referring to the Latin edition of Descartes' Geometry prepared by Schooten and first published in Leyden in 1649. It underwent four editions and contained several explanatory tracts, including Hudde's and De Witt's). Studying such authors as Vieta, Harriot and Anderson would also be valuable here.

Secondly, the student of mathematics might progress to the dimension of figures. Rivactus' editions of Archimedes' De Sphaera et Cyllindra, de Conoidibus et Spheroidibus, de Quadratura Parabolae and de Dimensione Circuli are the first step. Then follows the doctrine

of indivisibles, found in Cavalieri and extended by Torricelli. Next he should study Wallis' Arithmetica Infinitorum, Heurat's Epistola de transmutatione curvarum in rectas, James Gregory's Pars Matheseos Universalis and de Circuli Quadratura, Barrow's Lectiones Geometricae and David Gregory's Exercitationes de Dimensione Figurarum, along with as many other recent works as he could get hold of. Each work would be a little less obscure than the last as the student would now be growing into a true mathematician. None of Newton's writings on these topics were readily available when Gregory composed this paper. However, Leibniz had published several papers in the Acta Eruditorum on his differential calculus, and these had been followed up by continental mathematicians such as the Bernoullis and de l'Hôpital. Perhaps it was simply their relative inaccessability which led Gregory to omit these works from his reading list. Indeed, it seems to have been partly a desire to combat this problem which led Gregory to compose his tract on fluxions which applied Newtonian methods to the continental problems. (See Chapter 4.3).

The third addition to this paper, geometrical physics, culminating in the Principia, presupposed at least some acquaintance with these studies of Cartesian geometry and the dimension of figures. Certainly, the student who had enough physics and geometry to tackle Newton's book, was one who had studied these topics in detail.

Just as this private reading list extended the basic course, so the Christ's Hospital scheme abridged it. Wallis and Gregory agreed with the recommendations of the new scheme that arithmetic and Euclid were the essential basis of all further study. Algebra, too, would help to give the boys an insight into arithmetic and 'by these means

they are secured agt [against] forgetting their Arithmetick, a thing very ordinary'! Euclid's Elements might at first seem 'hard and tedious' yet the ease which a thorough knowledge of them would give to later work would amply recompense the student for these early struggles. Plane and spherical trigonometry, the art of projections and of making maps and charts, the doctrine of globes, the rudiments of geography, hydrography and astronomy with the application of all this to navigation make up the course. They agree, too, with the proposal to teach 'perspective and designing' and this is the only part not included in Gregory's under-graduate course.

Newton had recommended that the new scheme be extended to include mechanics⁶⁴, and Gregory and Wallis made a similar recommendation. The theory and use of the five simple machines and a knowledge of hydrostatics seemed indispensable for a seaman.

In emphasis, this paper differs from the others. Many topics which are additional options to the study of astronomy for Oxford under-graduates are essential topics for the would-be seaman of Christ's Hospital. The paper says that the pupils should understand the instruments used for observing the heavenly bodies, but this need not have included telescopes. There is no mention of optics as an independent field of study. Nor are algebra and mechanics to be studied in the depth expected of a university student. However, in basic principles, this scheme is very similar to that proposed for under-graduates.

All in all, these papers are most noticeable for their coherency.

⁶⁴ NC III 452 357-66.

As one might expect, the course of private reading goes somewhat beyond the two discussions of a suitable basic course for undergraduates, while the course for Christ's Hospital covers somewhat less. The basic under-graduate course discussed here was also that taught at Edinburgh in the 1680's.

Both in the Edinburgh lectures and in these papers on education, Newton's work occupies the same position. The papers outlining under-graduate courses do not mention Newton: certainly he is not a suitable author for the boys of Christ's Hospital. Only as the very last book in a comprehensive reading course of private study is the Principia mentioned. Even here it is with the caution that it should be read only if the student now has sufficient physics and geometry. His optical papers in the Transactions are not mentioned at all.

Gregory did not expect his students to follow Newton's work - he did not find it easy himself. Nor did it seem to him necessary that they should. The practical aims of Gregory's educational system in mechanics, optics and astronomy encompassed understanding the five simple machines and the elementary laws of impact and projectiles, the use and design of optical devices and the ability to plot the positions of stars and planets. Newton's theoretical work was irrelevant to these goals, and even more so to courses on practical geometry, logarithms and trigonometry. Thus, the few mentions of Newton in Gregory's Edinburgh lectures (with the partial exception of the hydrostatics lectures) are peripheral to the main theme.

2.5 The Edinburgh Lectures

These lectures largely follow the pattern set out in the papers on mathematical education. The four basic subjects were arithmetic, Euclid's Elements, trigonometry and logarithms and practical geometry. We have no lectures on the first two topics (unless we suppose the English arithmetic lectures are David's, which seems unlikely⁶⁵). Arithmetic, however, was the province of the lower schools and it would have been reasonable for Gregory to assume that, although some might benefit from private tuition, most under-graduates would already have a grounding in the topic. Euclid was taught by Gregory, as he says in the introduction to his lectures on practical geometry, but this would have been taught straight from the book, with no dictated notes. We have several copies of notes on the other basic topics; trigonometry and logarithms and practical geometry.

Next, the student studied algebra, astronomy, optics and mechanics. Again we have lecture notes on all these topics except for algebra. Perhaps Gregory never taught algebra, but left Edinburgh before he had time to include it in his repertoire of lectures. On the other hand, the books he lists for the study of algebra are more numerous than for any other topic, and it is possible that he found it sufficient to teach from text books without giving dictated notes. In addition we have his notes on one, possibly two, of the optional topics. The lectures on gnomonics form an addition to the astronomy and those on hydrostatics, whether they are David's or James', are an addition to the mechanics.

⁶⁵ SUL QA503 G8.

If these lectures are influenced by any one author it is Descartes. His ideas are not always uncritically accepted, but Cartesian elements appear in the optics, mechanics and astronomy. Newton's reflecting telescope is mentioned in the optics, but not his theories of light and colour. Newtonian ideas appear in the mechanics and astronomy only in the final, and most advanced, parts. (The earlier parts of both these courses were written before the Principia appeared.) Yet even here they are not discussed in any detail. More important were Mercator's Institutiones in the astronomy and Wallis' De Motu in the mechanics, though, especially in the latter case, other authors were also used. The lectures on logarithms, trigonometry, practical geometry and gnomonics consist of straightforward rules for calculation with explanations of their derivation. They are in no way remarkable and might have used any of several contemporary authors as their source. Only in the hydrostatics lectures, which were perhaps James', are Newtonian principles explicitly stated and an attempt made to use them as a basis for a course aimed at under-graduates. Yet even here, Wallis' expositions were found more suitable and the bulk of the lectures is taken from his work.

The lectures are not very original, although, as is seen especially in the optics, Gregory does not follow his sources slavishly. They are clear and competent, their worst fault being a failure to acknowledge sources, even when quoting them verbatim. In the context of Gregory's conception of the aims of an under-graduate education, this lack of originality is scarcely important and even his failure to acknowledge sources may be excused. Taken generally

they provide the mathematical education he describes in his papers; the topics he discusses and the way in which he deals with them mirror the opinions of these papers.

Finally, we have four graduation theses given by Gregory's students which provide our only insight into his private teaching. Most importantly, they show that his enthusiasm for Newton's Principia was passed on to his students, even if not through the medium of his public lectures.

Thus, the work of Newton, too difficult for, and irrelevant to the needs of, Gregory's pupils formed no important part of his lectures. The manuscript notes which helped educate Scottish students for decades barely mention Newton. It was only to his own students, whom he taught in private, that Gregory transmitted his respect for Newton's work.

2.6 Lectures on Optics

The first lecture course which Gregory gave was on optics, a subject in which there had lately been many advances. The law of reflection, that the angles of incidence and reflection are equal, had been known to the Ancients, and catoptrics had reached an advanced state in the Middle Ages. However, the law of refraction, that the sines of the angles of incidence and refraction bear a fixed proportion to each other, had not been so easily discovered.

Kepler's work on optics was contained in two works; Ad Vitellionem and Dioptrice⁶⁶. The first of these is remarkable for its theory of vision, in which he clarified the confusion then existing over the function of the parts of the eye. Kepler was thus able to explain the function of spectacles. After many experiments, however, the nearest approach he could make to the law of refraction was $i - r = c \times i \sec r$, where i is the angle of incidence and r of refraction and c is a constant.

The Dioptrice appeared in 1611, the year after Galileo's Starry Messenger had revealed the new discoveries which the telescope granted the astronomer. Kepler's work attempted to show how the telescope worked, and here he used another law of refraction. He recognized that the angles of incidence and refraction were not strictly in proportion, but he argued that this proportion might nevertheless be assumed for angles under 30° . As the rays of light enter a telescope very nearly along the axes of the lens, this assumption enabled him to

⁶⁶ Johannes Kepler Ad Vitellionem paraliponema, quibus astronomiae pars optica traditur ... (Francofurti, 1604) and Dioptrice (Augustae Vindelicorum, 1611).

explain the images thus produced, with only a small loss in accuracy.

Snell and Descartes ^{often} share priority for the sine law of refraction. ^{although Harriot had discovered it earlier.} Snell's experimental discovery was well-known to the scientific world in 1637 when Descartes published a 'physical' derivation of it in his Dioptrique⁶⁷. He applied the law to the problem of finding lenses free from chromatic aberration, and also discussed the nature of light and the means of vision.

Descartes' explanation of the sine law meant that light must be imagined to travel faster in a denser medium; Fermat found this unacceptable and devised an alternative deduction based on the 'principle of least time'. Using this method of maxima and minima, he showed that a ray of light travelled in the least time from one given point to another in a different medium if it obeyed the sine law of refraction in crossing from one medium to the other. This analysis implied that the resistance to light (and not, as with Descartes, its speed) was proportional to the density of a medium⁶⁸. This least time principle was to become important to Gregory, not only in the study of refraction, but in many other contexts.

Thereafter several texts on optics appeared, many of which reworked Kepler's explanations of the behaviour of light passing through lenses, and so examined the function of telescopes, in terms of the sine law of refraction. Such a work was Dechales' Dioptrica⁶⁹.

⁶⁷ La dioptrique was appended to René Descartes Discours de la méthode ... (Leyden, 1637).

⁶⁸ J.F. Scott The Scientific work of René Descartes (London, 1952) 40.

⁶⁹ Claudius Dechales Cursus seu mundus mathematicus 3 vols (Lugdini, 1674) ii 608-731.

Besides Descartes Dioptrique, however, the works which David Gregory mentioned in his suggested reading list were James Gregorie's Optica Promota and Isaac Barrow's Lectiones Opticae⁷⁰.

The Optica Promota was James Gregorie's first published work, and appeared in 1663. Written with the encouragement of his brother, Gregorie of Kinnairdie, it contained an independent discovery of the sine law of refraction. Proposition 5, using an 'ellipse of density' constructed in proportion to the refractive index between the two media, derives this law and then gives an experimental verification. Using this law, Gregory was able to give many results on the image of a visible object after refraction. As a development of his work on reflection, the scholium to proposition 59 gave the design of a reflecting telescope from which Newton took the idea for his own⁷¹.

James intended a second edition of this work. His own copy contains many notes in his own hand⁷² and he also produced a supplement which is now among the papers of his nephew David⁷³. This supplement begins with a fuller description of the mechanism of vision and goes on to look at spherical lenses and mirrors. The Optica Promota had dealt almost entirely in the more precise images occurring

⁷⁰ James Gregorie Optica Promota, seu abdita radiorum reflexorum et refractorum mysteria geometricè enucleata (London, 1663).
Isaac Barrow Lectiones XVIII ... in quibus opticorum phaenomenon genuinae rationes investigantur (London, 1669).

⁷¹ Newton to Oldenburg 4.5.1672 NCI 59 153-5.

⁷² GTV 458-9.

⁷³ C15.

on inflection in a conic section. It mentioned spherical lenses and mirrors as approximations to these surfaces, but with a warning that they would never be entirely satisfactory. Probably the failure of his attempt to have a suitable parabolic mirror ground in London⁷⁴ had convinced him of the need to construct adequate optical machines from spherical lenses and mirrors. In any case, this supplement ends by studying just this problem.

An interesting point in this paper is James' attempt to show that Kepler had used, more or less anyway, the correct law of refraction. His first assumption had been that the angles of incidence and refraction are in proportion, but he followed this later by approximating the ratio between the angles of a triangle to that between its sides (which are, strictly, as the sines of the angles). Thus, by some combination and cancellation of his two assumptions we have the sines of the angles of incidence and refraction in proportion. This admiration for Kepler's optics, and the wish to attribute as much to him as possible, was shared by David Gregory.

Isaac Barrow's Optical Lectures was a far more comprehensive work than Gregorie's. Published in London in 1669, it had been revised by Isaac Newton and edited by John Collins. In it, Barrow discussed exhaustively the inflection of divergent, convergent and parallel rays at plane and spherical surfaces. By applying the sine law of refraction and considering cases where the eye of the observer lies outside the axis of the lens or mirror on which the radiant lies, he extended widely the science of optics. His treatment was, of course,

⁷⁴ GTV 3.

geometrical and many of his problems had to be resolved into numbers of special cases before he could solve them. It was not until Edmond Halley's paper on finding foci appeared in the 1693 Transactions that the ease with which these problems might be treated algebraically (with due regard to sign) was appreciated. In this way Barrow's special cases became only one example of a general analysis⁷⁵.

Most of those treating mathematical optics also discussed the physical properties of light. Barrow and Dechales both did this. Others are remembered predominantly for their physical speculations. Hooke, for example, produced his two colour theory in the Micrographia⁷⁶. Light was a wave form and the basic colours, blue and yellow were distinguished by the obliquities of their wave fronts. Also, in several papers in the Transactions in the 1670's, Newton produced his theory of colour, based on differing refrangibility.

Thus when Gregory wrote his optics lectures he had a wide range of sources on which to draw. His treatment is similar to that of Dechales, in that he discusses problems much the same as those looked at by Kepler, but uses the sine law of refraction in doing so. However, he is mathematically more precise, and defines and uses the concept of focus in a way closer to that of Isaac Barrow. He does not try for Barrow's generality, but all the results Gregory gives might be found in Barrow. Descartes Dioptrique and James Gregorie's Optica Promota are also used in the course of the lectures, as are

⁷⁵ PT 17 (Nov. 1693) no. 205 960-9.

⁷⁶ Robert Hooke Micrographia (London, 1665).

the Philosophical Transations. David calls as well on his experiences when abroad and describes a magic lantern seen at Leyden.

However, the lectures are utilitarian. They were not designed to philosophize on the nature of light and that topic is left strictly alone. Instead, they study its behaviour and then deduce the functions of optical devices.

Some of the questions avoided in these lectures were answered later, however. In the lectures he gave for the degree of M.D. Gregory examined questions on the means of vision which he dismissed as irrelevant in these optics lectures. The Oxford lectures are examined in 2.6.2.

Finally, Gregory's text book on optics appeared in 1695, and was based on the lectures he had given at Edinburgh. It was geared to the needs of under-graduates and used by them for many years. This text, and its differences from the 1683 lectures, are examined in 2.6.3.

2.6.1 The Edinburgh Optics Lectures: Lectiones Opticae

These lectures deal with catoptrics and then dioptrics. In the latter part the mechanism of vision and the construction of optical machines are discussed. Reflection and refraction are both considered in plane and spherical surfaces, but not in conic sections. Nor is the nature of light considered a suitable topic for such a course, as Gregory makes plain at the outset.

'We dismiss philosophical questions of the nature of shining light etc. as irrelevant to our plan; content to suppose that vision is performed by the intromission of rays from the visible to the eye and not by any

emission of rays from the eye to the object, and that these rays emanate in all directions from any visible point (which we will afterwards call by the more apt name of radiant), that is, they are directed to every point of a diaphonous and uniform medium'.

Nor, he continues, will he consider whether light is the action of a body, or a kind of pressure transmitted from one particle to another to reach the eye. That is, he will not choose between the traditional view of light (to which Descartes himself had recourse at times) and the Cartesian 'pressure' hypothesis. Both, Gregory feels, rest on the same arguments and are beset with equal difficulties. He does not even mention wave theories such as Hooke's. He will, however, assume that in a uniform medium, light is propagated rectilinearly. Thus, his three initial assumptions were that light is the effect of something moving from a radiant body to the eye, that such rays are transmitted in all directions from every point of a radiant body and that, in a uniform medium, the transmission is rectilinear. All these assumptions had long been accepted by opticians. He assumed further that all inflections take place in a plane perpendicular to the inflecting surface, and pointed out that one might instead (as Kepler had) assume that a ray meeting a surface perpendicularly either continues unaltered or is reflected back onto itself. This, too, was a well-established axiom, and it was only such that Gregory was prepared to accept.

Not only did he refuse to discuss the mechanism by which light is transmitted, but he also refused to be drawn into any other controversial discussion, such as the validity of Descartes' analyses

of reflection and refraction, or the argument between Marriotte and Pecquet over the seat of vision. Both these issues are raised and the arguments briefly sketched in but, although he is clearly dissatisfied with Descartes' analyses, and also agrees that from an anatomical point of view it would be more logical that the retina was the seat of vision, he does not commit himself in either case. Nor would he discuss how two inverted images (one on each retina) are perceived in our mind as one upright one.

For his physical explanations of reflection and refraction, Gregory turned to Descartes Dioptrique. Here, the motion of an incident ray was split into two components, one parallel, and one perpendicular, to the inflecting surface. On reflection the parallel motion is unchanged, and the perpendicular motion reversed, giving the familiar law $i = r$. On refraction the parallel motion is again unchanged, but the ^{total} perpendicular motion is varied in a set ratio depending on the media involved. This gives us the refraction law, $\sin i = c \sin r$. Gregory points out, however, that in the latter case, this analysis implies that light travels faster in a denser medium. Barrow and Maignan had avoided this difficulty (by considering a line of definite thickness). Best of all was the method by which Fermat had resolved this matter, by making it a matter of geometrical analysis through his method of maxima and minima. This put the result beyond all doubt, but was extremely difficult. (Certainly, it was too difficult for Gregory's students.) Here, then, Gregory has not chosen the best or most convincing proof he knows of the refraction law. Instead he uses the one which it is easiest for his students to visualize and comprehend, and thus the

one which is best suited to their needs. In a similar spirit he would refuse some six years later to introduce Newton's lunar theory into the final part of his astronomy lectures.

As mentioned above, many sources might have been used in compiling the notes. Gregory may have derived his propositions straight from Kepler, using Barrow's work as a model for the techniques to employ. Certainly the descriptions of spectacles and telescopes are very like those in Kepler's Dioptrica. Alternatively, he may have used a modern author such as Dechales, whose treatment was similar though far more extensive. Even then, he probably used Barrow as a model. In particular, Gregory's use of the general refractive index $I : R$ rather than a particular index for say refraction from air to glass is akin to Barrow's usage. James Gregory's supplement, which uses much of Kepler's work, might also have been a source of ideas which might otherwise have come directly from Kepler.

James' Optica Promota discussed mainly refraction in conic sections which was outside the scope of David's lectures. However, it appears in the final scholium for its description of James' reflecting telescope. In his introduction to Dioptrics, David had argued the advantages of lenses over mirrors; they do not tarnish or rust, and the loss of light on refraction is less than that on reflection. Somewhat contradictorily, then, he added in his final scholium that the best telescopes of all were those made of mirrors and lenses combined together. Proposition 59 of James' Optica Promota describes such a telescope, the invention of which was unwittingly claimed by Cassegrain for himself sometime later. 'Clarissimus' Newton has made a model of this telescope which, he tells his students,

is in Gresham college (where Gregory had seen it on his trip to London in 1681⁷⁷.) He will not describe it here, as this is done in the Transactions.

Even here, when to do so would save him from self-contradiction over the merits of lenses and mirrors, Gregory does not mention Newton's theory of colours and the consequence of chromatic aberration. This sole reference to Newton, as the man who made a model of James Gregory's telescope, is the only one in the Optics lectures, and the only one which we find in any of the lectures before 1688, when Newton was mentioned in the mechanics lectures.

Descartes' Dioptrique had furnished Gregory's physical proofs of the laws of reflection and refraction. The description of the parts of the eye and their functions was probably also taken from the same source. There is no important difference between the descriptions given by Gregory and Descartes nor between their figures. Of course, other authors had used this explanation and it may have come to him from other sources, but, since he was able to quote from it verbatim in his treatment of refraction, Gregory had access to the Dioptrique, and he must have been aware that his description of the eye was Descartes'.

Other sources are revealed in these lectures. The argument between Marriotte and Pecquet had appeared in the Philosophical Transactions⁷⁸. On his visit to Leyden in 1680, Gregory had seen a magic lantern which threw pictures onto a wall, and had made a drawing

⁷⁷ c9

⁷⁸ PT III (May, 1668) No. 35 668-9, 669-71, PT V (May 1670) No. 59 1023-42.

of this marvel for himself⁷⁹. Three years later he described it to his students. Other comments, such as the reference to Hooke melting down glass to make lenses more easily, and of a Mr. Melin of England who made lenses so small that they could only be seen with a microscope, do not seem to have appeared in any source with which we know Gregory was familiar. Probably Gregory had gathered these pieces of information when he was in London in 1681.

These lectures are primarily concerned with the use of optical machines, and they reach their aim by the most appropriate means at each stage. There is no experimental approach; Gregory, says, for example, that although the law of refraction is established by experiment, students of natural philosophy will prefer a physical explanation. Yet the explanation was not the most convincing, but the one most suited to his students' abilities. He tells his students that a sight of a real magic lantern would let them understand the machine better than they could from any description, but there is no hint that he intended to try and demonstrate its principles himself.

Mathematically, of course, the lectures are perfectly competent. When considering inflections in spheres a degree of approximation is necessary and Gregory apologises for using methods of less than geometrical exactness.

Gregory could be justifiably pleased with his first session of lectures as Edinburgh's mathematics professor. He explained clearly and concisely all that was necessary for an understanding of the aids to vision. The problems neither of spherical nor of chromatic aberration were mentioned, but an interested student who had followed

⁷⁹ C159.

Gregory's course would be in a position to understand these problems easily. More importantly, the less interested, duller student would be perfectly able to understand spectacles and microscopes without a knowledge of these reasons for their imperfections. These lectures serve admirably the practical ends for which they were written.

2.6.2 The Lectures on Galen

When Gregory was made Savilian Professor of Astronomy, the University of Oxford granted him the degree of M.D. Presumably this was as much an honour to the position, generally held by doctors of Divinity, as to Gregory. He had no claim to a doctorate in Divinity, Law or Music, but through his family background and through his friendship with Pitcairne he had acquired some medical knowledge. It seems highly unlikely that he had had any formal training in the topic, for there is no reference in his papers to such studies. However, it is possible that his journeys on the continent were connected with some study of medicine, but that his papers on this topic were kept separate from his others and have been mislaid. Whatever his qualifications, Gregory was awarded his M.D. and on the 9th, 10th and 11th March 1692 he gave three lectures on Galen to justify this honour.

The influence of the iatro-mechanical school, almost certainly as exerted by Archibald Pitcairne is at once apparent. He begins

'I resolved in these lectures or exercises for the degree of Doctor of Medicine of which the University has thought me worthy, to investigate for my part a certain physical question which in common opinion depends little on the mathematical sciences; so that I might show their

usefulness in medical studies and at the same time make a test of our geometry in something scarcely abstracted from matter; and I decided at first glance, therefore, to leave optics which geometry has been well-known to aid for a long time; and to approach something new, but perhaps no less subject to the laws of geometry, as the motion of muscles, the secretion of animals, or something similar.'

However, the statutes of the university decree that a candidate for the degree of Doctor of Medicine must show his ability by commenting on Galen and this Gregory is constrained to do. He chooses to discuss books four and ten 'De Usu Partium' and 'De Locis Affectis' and for this purpose the eye and the means of vision furnish the most suitable example.

The three lectures are divided as follows; first, he discusses the external parts of the eye, secondly the manner of vision, and thirdly diseases of the eye. In the first part, Galenic principles are clear, when Gregory adopts his 'argument from design', set out in De Usu Partium⁸⁰. Gregory does not continue the argument by saying that the perfect adaptation of the forms of parts of the body to their functions proves the existence of a benevolent God. However, he tells us that each part has been formed so as to be most suitable of all for the function for which it is intended. The eyelashes, for example, are perfectly formed for protecting the open eye, while the eyebrows have been so fashioned that they prevent sweat running into the eye.

⁸⁰ See Lynn Thorndike A history of magic and experimental science 8 vols (New York 1923-58) i 149.

(That he does not emphasize God's role here is not to deny his belief that the perfection of the material world provides our best proof of a divine presence. He had used this very argument three years previously to answer charges of atheism made against him before the committee of visitation⁸¹.)

This examination of form, as determined by function, follows Galen's order in discussing the fluids in the eye. Gregory mentions their refractive indices and the mechanisms whereby their distances may be altered, and here he corrects Galen's view of the relationship between the cornea and the crystalline humour. He thus gives a qualitative account of how an image is displayed on the retina; similar to that given in the Edinburgh optical lectures but without the previous mathematical treatment of refraction. He frequently compares the parts of the eye in man with the corresponding parts in fish, birds and animals where a different function gives rise to a different form. By implication, he had himself examined such eyes. This is especially so when he discusses the perfect refraction through a cow's eyes, even at the edges.

His knowledge of the iatro-mechanists appears again, when he discusses the suggestion of a recent author who compares one of the muscles governing the eye to a pulley. Another modern author had attributed the apparent size and twinkle of heavenly bodies to a motion of the pupil, and Gregory is quick to disparage this idea.

The third of the lectures is based on Galen's 'De locis affectis' and considers diseases of the eye. Gregory discusses ulcers or scars

on the surface of the eye, discoloured corneae, escape of aqueous humour through a wound in the cornea, cataracts and their early symptoms and generally obscured vision. Short and long sight are discussed, but spectacles and other artificial aids to vision are only briefly mentioned. Although he occasionally mentions surgery, Gregory does not give any medical treatments for these conditions.

However, it is the second lecture, on the manner of perception, which most concerns us here, and it displays Gregory's ability to tailor his discussion of a subject to fit his audience. In the third lecture he had dismissed the exact construction of spectacles which he had discussed fully in his optics lectures. Similarly, in the first lecture he had omitted any geometrical demonstration of the way in which the humours of the eye refract light rays to form a picture on the retina. In the second lecture, on the other hand, he discusses several of the more physiological problems of vision which he had dismissed in the earlier lectures. He also shows us that he did not maintain a total neutrality on all controversial topics, as the Edinburgh optics lectures might have suggested. Here, he decides clearly between alternative viewpoints.

In his Edinburgh lectures, although it was clear that he sided with Pecquet in his controversy with Marriotte over the seat of vision, he had not argued the case, nor discussed it in detail. He chose to leave such questions on the physical make up of the eye to the physicians. Further, he intimated that an explanation was possible for the appearance of a single upright image proceeding from the two inverted images displayed on the retina, but left this problem for philosophers. He did not mention the problem of judging distances.

Now, these three topics form the basis of his second lecture on Galen. So far we have not entirely discovered the way in which God arranged that our minds and bodies should interact, and so there is much about vision that is unclear to us. However, leaving more philosophical speculations, Gregory turns to describe what we do know of the transmission of vision to the eye, and what we may infer from this knowledge.

It has been observed that in animals whose eyes are so placed that they can see a nearby object with both eyes at the same time, the optic nerves leading from each eye are united into one. This does not happen in animals whose eyes are on either side of their head. The pencil of rays focused on any one point of the retina is then transmitted to the brain through an individual filament of this nerve.

The problem of inversion of the image is, Gregory believes, simply overcome. The eye does not judge up or down, left or right by the relations in the image thrown onto the retina, but by the movements of the muscles necessary to bring an object into the direct line of vision.

To judge distances with one eye only we need fairly subtle geometry. The eye uses the tiny alterations made in the crystalline humour and its distance from the retina to produce distinct vision. From these, the distance of an object may be calculated. However, these variations are so small that precise distances cannot be judged in this way. Alternatively, granted uniform illumination, distance is inversely related to the apparent brightness of an object. Size, he says later, is determined by considering the distance of an object and the angle it subtends at the eye.

The optic nerve is inserted into the back of the eye, but not, as one recent author claims, in the axis of the eye. It is certain from experiment that there is no vision at the spot where this nerve is inserted, either because there are no nerve fibres there, or because there are too many (Gregory inclines to the latter view.) It would clearly be bad design to place this blind spot at the axis of the eye where vision is otherwise most distinct. This blind spot is compensated for by the motion of the eye, which renders it unnoticeable. Marriotte's experiments have proven the blind spot exists, but it is wrong to infer thence that the choroid is the seat of vision, and now Gregory argues why the retina should be preferred.

Next he discusses the consequences of the join and subsequent separation of the two optic nerves. Does this merge the images of the two eyes and so produce the single image perceived by the brain? Gregory examines at length the arguments for and against this supposition, finally deciding against it. Instead, corresponding parts of the retinae are recognised in the mind as representing two images of a single object, and so it is the mind which converts the double image into a single one.

Vision is clearer with two eyes than with one, and the estimation of distance is much improved by utilizing a process of triangulation. Finally, Gregory explains why, whether we look at near or distant objects, the two images of the one object cannot both fall on the blind spot of each eye.

Clearly Gregory knew far more of the manner of vision than he had dictated to his Edinburgh students in 1683. Of course, these lectures on Galen were given almost ten years later and he may have

read more widely in the intervening years. It seems certain that he would make an effort to be abreast of modern ideas in the subject before giving these medical lectures.

However, when he published his Elementa Opticae, an undergraduate text book of optics, three years later, the structure of the eye and the means of vision were not mentioned. Rather than including the discussion he had given for his M.D., he even omitted most of what little had been said on these topics in the Edinburgh lectures. To Gregory, they were irrelevant to an under-graduate who wished to study optical machines - the obvious goal of an under-graduate course in optics. Although he was interested in these matters and knowledgeable about them, able to decide between conflicting theories and to form his own opinions, he did not consider them suitable for under-graduates.

Once again we see that Gregory's omission of Newtonian philosophy from his lectures was not a special case. His lectures consisted of the studies he thought useful to an under-graduate, not of his own enthusiasms and researches.

2.6.3 Elements of Catoptrics and Dioptrics

Some time after his appointment at Oxford, Gregory began to put together a text book on optics, suitable for under-graduates. This may have been prompted by a desire to supply a lack among such texts, or by a wish to prove himself further as an author. In either case, the lectures he had given at Edinburgh provided a suitable basis for such a work.

We do not know when Gregory began to write this work. He mentioned it to Newton when he visited him in May 1694 and he completed

the first draft in July 1694⁸². An amanuensis copied out this draft, incorporating the many alterations and amendments which Gregory had made to it. This second draft, which is now unfortunately incomplete, was further altered and added to by Gregory to produce the published version⁸³. A first draft of the preface was drawn up on 24th October 1694⁸⁴ and the imprimatur was granted by Henry Aldrich on 18th April 1695.

When planning the work, Gregory noted down eight points he would change in the optics lectures. The definitions were to be re-examined using Kepler's Dioptrics, and their number to be cut down. The treatment of optical machines was to be revised and the determination of images extended somewhat. It was not, however, to include lenses refracting rays geometrically; that is, free from spherical aberration. The only alternative to omitting this altogether (since it had been considered by Barrow) was to name Barrow and point out his errors in this problem. Gregory found the first alternative more pleasant. He intended at this stage to use his lectures on Galen for the discussion of how our mind perceives one erect image from the two inverted ones on the retina. However, he must have realised the irrelevance of such discussions to his aim, for the means of vision are scarcely mentioned in the book. The preface to the optical lectures might well provide a preface for the text book.

However, the most interesting point is the first one

⁸² 5-7.5.1694 C43 RG fo. 68 NC III 444 327-8; B18, dated 'Vesp: Comitiorum Acad: Oxon. MDCXCIV' Commencement day was at the beginning of July.

⁸³ B18.

⁸⁴ Misc. 3.

'The ever constant measure of refraction in the same media [is to be deduced] not from the system of any philosopher, but universally, just as we did from proposition 20 Chapter 1 of Kepler's Paralipomena in Vitellionem. I have also deduced that these matters do not really belong to Dioptrics but to philosophy. The same is also to be done in reflection'⁸⁵.

In the Edinburgh lectures, Gregory had clearly been dissatisfied with his use of Descartes to explain refraction, but had judged it acceptable for his purpose. However, at some point, in an unfortunately undated paper, Gregory re-examined Descartes' argument⁸⁶.

Here Gregory showed that when we split the motion into components perpendicular and parallel to the inflecting surface, we keep the parallel component constant, but the alteration of the perpendicular component is such that the total resultant motion varies its speed in a set ratio - not the perpendicular component^{as he mistakenly supposed Descartes to hold}. If we do alter the speed of this component in a set ratio the sine law of refraction does not result.

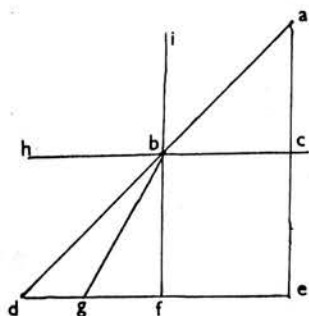
Of course, such a model is not altogether intuitively satisfactory, but Gregory seems more confused than one might expect. He claims that Descartes has split the motion into two components, but not the velocities. Yet he acts as if he had done so when letting the parallel component remain constant. The criticism in this paper is not always quite clear, but criticism it certainly is! We do not

⁸⁵ Between C115 and C116.

⁸⁶ C164 RG fo. 104.

know the date of this paper, but it seems likely that it was the perception of these flaws in the analysis which led Gregory finally to discard this model of refraction. He then looked for another suitable model to use.

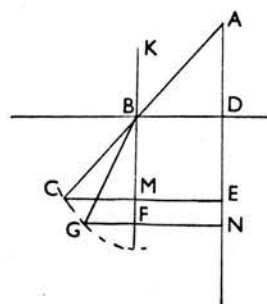
Luckily, such an alternative was to hand. Kepler's Paralipomena ad Vitellionem examined the behaviour of light rays on inflection in propositions 18 - 20. In particular, proposition 20 discussed the problem of refraction in a plane surface⁸⁷.



Where hc is the refracting surface and a the radiant point, Kepler considers the dispersal of the rays between ac and ai . In as much as the second medium is denser, it hinders this dispersal of the rays. That is, if bc measures their dispersal on arrival at the refracting surface, de would be its measure when ac had reached e if the two media were the same. Because of the greater density of the second medium, however, this dispersal is hindered, and the measure of dispersal is only ge . Thus, ab is refracted into bg . Kepler uses this only to argue that light rays are refracted towards the perpendicular when they enter a denser medium, but Gregory, with a prior knowledge of the sine law of refraction, was also able to extend this argument.

Consider again all the rays between AB and AD , and suppose that AB is refracted at the surface BD into BG , where BC is the path it would have taken had it not been so refracted. That is,

⁸⁷ Kepler op cit (66) 15-21.



the light which was dispersed into BD at the refracting surface and would have been dispersed into CE had there been no refraction is, in fact, dispersed into GN after refraction, where BC and BG are the same length.

Since FN is independent of the readiness of the second medium to allow light to disperse, we may omit it from both CE and GN, so that CM measures dispersal in the first and GF in the second medium. That is, since 'effects are proportional to causes' the ease of dispersal in the first medium is to that in the second as CM is to GF, or as $\sin \hat{CBM}$ to $\sin \hat{GBF}$. But $\hat{CBM} = \hat{ABK}$ is the angle of incidence and GBF is the angle of refraction, while the ease of dispersal in the two media is (assumed to be) independent of the angle of incidence. Thus we arrive at $\sin i = k \sin r$.

Gregory further justifies the omission of FN from the two measures of dispersal, by pointing out that the length of AB has no effect on the refraction which the ray undergoes. Thus we may take it as zero, in which case FN is also zero and GN the total dispersal becomes GF which is thus 'used by Nature as a Measure of the facility [i.e. ease of dispersal] of the medium DG to which it owes its existence'. We would be able to use a tighter mathematical statement of this argument today, but Gregory's meaning is clear.

There is a more deceptive point in his argument which needs clarifying, however. There is no a priori justification for considering the cases as corresponding when $BC = BG$. Indeed, it is far more logical to take, as Kepler did, equal lengths of the unrefracted ray AD, for here is a quantity apparently independent of the media. (Of

course, its speed is changed, but speed is not discussed or even mentioned in Gregory's analysis).

The case which Gregory takes yields the correct law, just as Descartes' equally arbitrary assumptions had done. As so often in his work, Gregory has produced a convincing and elegant argument leading to a valid conclusion, but the argument is one which would never have been devised without a prior knowledge of the required conclusion. Gregory's answer is more satisfactory than Descartes', since it avoids the faster motion of light in denser media. Yet it is just as much the product of arbitrary assumptions such as Gregory had criticised in Descartes when he wrote on his refraction analysis.

Perhaps it was partly because Fermat's analysis on the basis of the least time principle seemed to avoid such assumptions that Gregory found it so attractive. However, it was too difficult an analysis for him to do more than mention it approvingly in the Edinburgh lectures.

However, early in May 1694, while he was completing the Optics text, Gregory visited Newton at Cambridge and they discussed various topics including this work. (See Chapter 4.2) Probably with this in mind, Newton gave Gregory his reworking of Fermat's analysis⁸⁸. The paper containing this proof is among Gregory's papers in Newton's hand. The principle is precisely that of Fermat, but using fluxional terminology, notation and methods, Newton was able to render the argument in a much more concise form. Gregory used this paper for his 'Tract on Fluxions' which he wrote in the autumn of 1694. It was also added by him to the amanuensis copy of the Opticae Elementa and

⁸⁸ C38 RG fo. 165.

published therein. Gregory refers the method of fluxions used to Isaac Newton, as seen in Wallis' Opera, but he does not state that the example itself was given to him by Newton. This practice of Gregory's (in which Newton must have concurred) of presenting Newton's work as his own was to appear most markedly in the Astronomiae Elementa (see Chapter 5), but it is already apparent in the Opticae.

Newton possibly also influenced Gregory to discuss the priority issue surrounding the laws of refraction. In the scholium to proposition 96, book 1 of the Principia⁸⁹, Newton had remarked that Snell discovered that refraction was performed in such a way that the secants of the complements of the angles of incidence and refraction were in a constant ratio, and Descartes had shown that there was therefore a constant ratio between the sines of the angles themselves. In January 1693, Gregory commented in his Notae on the scholium that he had seen a book in the Bodleian library, which he later identified as Isaac Voss', which attributed this discovery to Snell. However, the author of this work seemed to understand little geometry and 'Descartes is always ill-spoken of by those of Oxford or Cambridge'⁹⁰. In another note made at about this time, although he is still dissatisfied with Voss' geometry and wishes there were more evidence than Voss' word alone, he seems convinced that Snell had found the law, from which Descartes might have derived his version⁹¹. Finally,

⁸⁹ Isaac Newton Philosophiae naturalis principia mathematica (London 1687) scholium to prop. 96, book 1 p.231.

⁹⁰ Notae 55.

⁹¹ A38.

he tells us in his Optics (inserted as an addition to the first draft, and so probably added after his visit to Newton in May 1694) that, as Voss tells us on page 36 of his De Natura et Proprietate Lucis (Amsterdam 1662) Snell had first known this law. Voss had seen it in his papers, and Snell had died ten years before Descartes, to whom the law is commonly attributed, had published it. Thus Gregory's opinion developed from the implication that the harshness with which Descartes was viewed by Oxford and Cambridge men had influenced them into believing the evidence of Voss, himself a poor geometer, to the firm statement that Snell had known the law long before Descartes. This may reflect Newton's growing influence (for in 1693 Gregory had found it possible to be doubtful over the firm opinion expressed in the Principia), or it may reflect Gregory's growing distrust of Descartes' derivation of the law.

Further, Gregory pointed out that Kepler had used secants as the particular measure of refraction. In his 'supplement' James Gregory had supposed that Kepler had nearly fallen on the correct law because, in the Dioptrice, he first approximated the law by putting the angles of incidence and refraction proportional, and then approximated angles by their sines⁹². David looked instead at the Paralipomena where in propositions 5 and 6 Kepler mentioned secants in proposing a law of the form $i - r = c i \sec r$ ⁹³.

Unfortunately, Browne's translation rendered Gregory's

'Keplerus ... secantes hasce adhibuit pro refractionum

⁹² C15.

⁹³ Kepler op cit (66) 112, 3.

mensura partiali'

as 'Kepler ... lays down these secants for the respective Measure of Refractions'⁹⁴.

It was largely this mistranslation which led to the criticism of Gregory's *Optics* in the Biographia Britannica, discussed in 2.6.4.

The alteration in the treatment of refraction and reflection (for the latter was given a similar 'Keplerian' discussion) is the most important difference between this book and the Edinburgh lectures on which it was based. The discussion of vision was largely omitted, as was, for example, the description of the magic lantern, for its principles were by then well enough known. There was however, some introduction of an algebraic treatment in the Optics, which was not in the lectures. The relationship between a spherical surface and those with which it is equicurved was made more explicit. The construction which Barrow had received from a friend, Newton, for his Optical lectures is repeated by Gregory, and finally there was the hint about the construction of achromatic lenses.

The algebraic treatment is restricted to two propositions, 20 and 24, neither of which had appeared in the lectures. In proposition 20, it is proposed to find the position in which a radiant should be placed so that it has a given proportion to its image with respect to a given lens. Gregory was able, by expressing the formula algebraically, to give several cases in one equation. Halley's paper in the 1693 Transactions had treated algebraically the problem

⁹⁴ David Gregory Catoptricae et dioptricae sphoericae elementa (Oxonii 1695) 46.

Dr. Gregory's Elements of catoptrics and dioptrics ... [with appendices and introduction]... translated by W. Browne (London 1715) 55.

of finding the foci of lenses. The paper had emphasized that, by using algebra, Halley could, in one theorem, encompass many cases which would previously have been treated separately⁹⁵. Gregory's proposition 20 was simply another application of the same principle. Proposition 24 introduces the problem of finding a glass mirror which will reflect parallel rays to an exact focus, that is, so that the reflected image of the surface nearer the radiant (whose curvature is given) and the image reflected by the further surface (with due regard to refraction in the first) coincide. Gregory gives two constructions for this depending on whether the thickness of the mirror is significant or not. He does not prove the constructions, but gives an algebraic formula from which they may be found. This formula, he says, may be derived from three other propositions in the book, each of which was handled geometrically.

At the end of this proposition 24 he mentions Halley's work. He had intended to give a general rule for finding the foci of all mirrors and lenses. However, Halley has already done this for lenses in the Transactions so Gregory does not give his. I have found no such rule among Gregory's manuscripts, although he does refer to 'his' method. This is used in an undated paper to find the focus of parallel rays in a reflecting mirror, but as far as can be judged from these somewhat scrappy calculations, it does not appear to be a general rule such as is mentioned in the Optics. Nor is it very clear why Halley's publication of the rule for lenses should prevent Gregory giving his more general one for lenses and mirrors! Perhaps

⁹⁵ op cit(75)

his 'intention' covered not only the publication of such a law, but also its discovery!

Whatever this rule was, Gregory nevertheless used geometrical methods, as he had in the lectures, for almost all of his propositions, with no algebraic generalization. For example proposition 15 distinguishes 8 cases of finding the focus of divergent rays refracted at a spherical surface. The position of the focus is constructed in the same way in each case, but the proofs (when detailed geometrically) are somewhat different. Gregory, instead of using an algebraic technique, proves only one case, in the traditional way, leaving the others as corollaries of this case.

In the Edinburgh lectures, he had declined to discuss surfaces formed from conic sections, considering only spherical surfaces. However, in the Optics he was careful to point out in detail how we could consider the equicurved sphere instead of these surfaces. Using Vincenzo Viviani's Maxima and Minima⁹⁶ he gave constructions for such spheres and frequently reminded his readers of this possibility.

One of the last additions made to the Optics as an insertion into the amanuensis copy was the construction Newton had given Barrow. The problem was that of finding the focus of divergent rays after refraction in a given lens. Gregory originally treated the problem merely by pointing to previous propositions which showed the reader how to find the focus after refraction in each surface. Later, perhaps on Newton's suggestion, but certainly with his permission, Gregory added Newton's construction as a corollary, with due reference to its place in Barrow's Optical Lectures.

⁹⁶ Vincenzo Viviani De maximis et minimis geometrica divinatio (Florence 1659) lib. 1, prop 20, cor. 1 p.43.

Perhaps the feature of Gregory's Optics which is most frequently mentioned is the hint about the construction of an achromatic lens⁹⁷. This is found in the final scholium of the work, which discusses the reflecting telescope of Isaac Newton and James Gregory. It points out that their advantage arises because of the differing refrangibility of light (a point omitted in the Edinburgh lectures) but gives no details of this discovery of Newton's, beyond saying that it may be ignored in small lenses. Continuing, he speculates that this problem might be overcome by forming lenses from two materials of different refractive index. So nature, who does nothing in vain, has constructed our eyes. In fact, there is not much here on which to build the concept of the achromatic lens, and certainly no examination of the practical difficulties involved. There is, though, a hint of the way in which such a lens might be found.

However, as Dr. Whiteside suggests, it seems more likely that this hint was taken from Newton⁹⁸. Zev Belcher has recently shown that Newton had in fact looked into the problem of constructing a lens of two materials with different refractive indices, which would be free from chromatic aberration. Although Newton was later to deny the possibility of such a lens, Belcher produces evidence to suggest that

'Newton was not unaware of the expression for the

⁹⁷ See, for example, Agnes Grainger Stewart The academic Gregories (Edinburgh 1901) 62-3.

⁹⁸ D.T. Whiteside 'David Gregory' DSB V 520-2 p.521.

compound achromatic lens⁹⁹.

He also proposes convincing personal rather than scientific reasons why Newton should deny its feasibility in the Optics.

Gregory visited Newton in May 1694 and wrote that scholium at some time in or before the beginning of July. The idea that Newton did give him this clue is inescapable - as is the fact that Gregory himself followed the idea no further. I have found no calculations which might refer to such a lens, nor any further reference to it among Gregory's papers. Of course, had the suggestion come from Newton, either with a hint that it was not in fact feasible, or with the information that Newton was working on it himself, either circumstance would have deterred Gregory from making his own investigation.

2.6.4 Reactions to Gregory's Optics

The first reaction to the work of which we know was undoubtedly enthusiastic. Charles Oliphant had been one of Gregory's first pupils at Edinburgh, graduating in July 1684 under Alexander Cockburn. He later took the degree of M.D. and was almost certainly the Dr. Oliphant, brother-in-law to Gregory, who was a life-long friend, and present at his death¹⁰⁰.

Among Gregory's papers, there is a proposed preface for the book, written by Charles Oliphant¹⁰¹. After discussing the lack of an optics text which was complete in essentials, yet not weighed down by

⁹⁹ Zev Belcher ' "A less agreeable matter", the disagreeable case of Newton and achromatic refraction' British journal for the history of science 8 pt.2 (July 1975) no. 29 101-26, p119.

¹⁰⁰ See Stewart op cit(97) 74, 5.

¹⁰¹ C114.

the author's verbosity, he tells us how he and others persuaded Gregory to remedy this lack by publishing those Optical lectures which Oliphant had himself attended as an Edinburgh student in 1683. The lectures were expressly designed to explain the properties of spherical lenses and so of optical machines, and Oliphant says of Gregory's work

'I do not remember having seen anything on this matter more beautifully proven or more well said!

There was nothing redundant included, and yet everything essential to understanding optical machines was to be found there. He apparently found it necessary, however, to excuse Gregory's limitation to spherical surfaces. Since the discovery of the sine law of refraction, says Oliphant, all optical writers have followed Descartes in concentrating on finding surfaces which will refract a radiant visible to an exact image. But these efforts are futile. First there is the difficulty of making such lenses, secondly, in the case of a relatively nearby visible, they will only refract precisely the point on the axis of the lens, and thirdly, as Newton has shown, the differing refrangibility of light rays means that chromatic aberration is inescapable. At this point, Oliphant rather abruptly wishes his readers farewell. This preface was never published, but it constitutes a powerful recommendation of Gregory's text.

The anonymous review in the Transactions is somewhat less strongly stated¹⁰². It gives an account of the subjects tackled in book, including the derivation of the laws of refraction and reflection

¹⁰² PT 19 (Jan, Feb, 1696) no. 219 214-5.

as Gregory had himself phrased it

'without restraining himself to any Sect of Philosophers'.

The reviewer concludes that

'The whole written with an accuracy and judgment worthy of its Author, does well merit the esteem of the Curious and knowing in Optical Matters'.

That is, the work was quietly, but well, received, with esteem rather than extravagant praise.

A second Latin edition appeared at Edinburgh in 1713, and in 1715 Browne published an English translation of the work at London, with the help of Desagulier and William James in various appendices. Browne tells us that he has

'all along in the Translation explained such Passages as the Author's Laconick Style has made too puzzling for a Beginner'¹⁰³.

This is the task of the first two addenda, where additional explanations and, more generally, proofs of constructions merely stated by Gregory, are set out. In particular, Browne gives general methods of finding the foci of any mirror or lens, as Gregory had once intended to do, until forestalled by Halley. Finally, there is an account of microscopes and telescopes by Huygens. In all, only a little over half of this edition constitutes Gregory's text, and we must remember this when the work is mentioned after 1715.

A further edition appeared in 1735, edited by Desagulier, who

¹⁰³ Gregory op cit(95) (1715) Addenda 1.

added a further appendix on the reflecting telescope, where he printed the letters of Newton, James Gregory and others relevant to its first discovery.

Daniel Waterland's 'Advice to a Young Student', first written around 1706 and printed in a final revision in 1740 does not list Gregory's Optics. Indeed, the only text specifically on Optics which he mentions is Newton's Optics, but he suggests works such as Rohault's Physics which included a section on optics¹⁰⁴. In 1707, Robert Greene published a pamphlet on a 'Method of Instructing Pupils' and he suggests Gregory's Optics for his third year students, along with Rohault, Dechales, Barrow, Newton, Descartes, Huygens, Kepler and Molyneux¹⁰⁵.

Rouse Ball specifies Gregory's Optics as one of three optics texts generally used by Cambridge students around 1730¹⁰⁶. The other authors mentioned are Newton (his Optical Lectures) and Robert Smith. This Smith was a cousin of Roger Cotes and succeeded him as Plumian professor. His Optics was published in 1728 and was very well thought of by Ball. It was a far more comprehensive work than Gregory's, consisting of four books; the first a popular introduction based on experiment, the second a mathematical treatment of the basic principles, the third a description of optical machines and the fourth

¹⁰⁴ The works of the Reverend Daniel Waterland (Oxford 1823-8) 6 301-24.

¹⁰⁵ Christopher Wordsworth Scholae academicae (Cambridge 1877) Appendix 4.

¹⁰⁶ W.W. Rouse Ball A history of mathematics at Cambridge (Cambridge 1889) 91.

a collection of the principle^{al} astronomical discoveries made by the telescope¹⁰⁷. However, there is no evidence that this book superceded Gregory's. On the contrary, the second English edition of Gregory's work was published seven years after Smith's had appeared.

The Optics seems to have enjoyed a modest popularity as a University text book for some time, perhaps partly due to the appendices to the later editions. In 1796, Hutton still described it as 'this valuable treatise'¹⁰⁸.

However, the author of the article on David Gregory in the Biographia Britannica, published in 1757, was severely critical of the work. He allowed that it was a good book, with nice and easy constructions, but believed that Gregory followed Kepler too closely in assigning physical causes to the behaviour of light. Since this was precisely the thing which Gregory believed he was not doing, this criticism is at first somewhat puzzling¹⁰⁹.

Gregory had begun his Optics, as he had the lecture course, by stating his intention to avoid all questions on the nature of light. In four places, the author in the Biographia finds he has not lived up to this promise. First Gregory says on page 2 that light rays in a uniform medium 'are propagated in straight lines (since [these are] the shortest)'. Secondly, and more seriously, Gregory talks of a

¹⁰⁷ Robert Smith A compleat system of optics in four books ... (Cambridge 1738) (first edition, 1728).

¹⁰⁸ Charles Hutton Mathematical dictionary - 2 vols (London 1796) i 555.

¹⁰⁹ Biographia Britannica (London 1757) IV 2367-9.

ray which falls perpendicularly onto an inflecting surface. This, he says in his first axiom, will either be reflected back onto itself, or continue its motion with direction unchanged. Instead of leaving this as an axiom he tries to explain it by saying this path is either the least (in the case of a plane, or some curved, reflecting surfaces) or the greatest (in other curved surfaces) line which may be drawn from the point to the surface. In each case the ray is unique and so there is no reason for it to choose one new direction rather than another. As such it must retain its initial direction or reverse it.

The Biographia approves of the 'Keplerian' demonstration of the laws of reflection, but not of the following scholium. In this scholium, Gregory says that much attention is to be paid here to maxima and minima. For, at a plane surface the path taken by a reflected ray between two points is the minimum such path

'since Nature's method of operating is the easiest and most expedite'.

In reflection at a curved surface, however, this path may be a maximum or a minimum

'for geometers know how close is a maximum to a minimum, a difference to a sum and how easily one may pass from one to the other'¹¹⁰.

Of course, when applied to curved surfaces in general, this claim is quite untrue, and the Biographia's comment 'what egregious trifling!' is not altogether unjustified! Similarly, the author disapproves of

¹¹⁰ Gregory op cit(94) (1715) 7.

Gregory's treatment of refraction by Newton's version of Fermat's analysis. Gregory proceeds here, he says, 'with like want of geometry',¹¹¹.

The justification for criticising these four points is discussed below. The Biographia also had some general comments on how Gregory glorified Kepler and denigrated Descartes, both beyond their deserts.

We saw above (2.6.3), how Browne mistranslated Gregory's 'Kepler ... used secants as a partial measure of refractions' as 'Kepler ... lays down those secants for the respective Measures of Refractions'. The author in the Biographia is clearly working from Browne's translation for he takes pains to point out that Kepler's use of secants was only a partial one, and suggests Gregory has been carried away by too great a reverence for Kepler.

Further, the author feels Gregory was unfair to Descartes. This is because it is pointed out in the Optics that Descartes, who otherwise considered light to be instantaneously transmitted, had assumed in his discussion of refraction that light travelled faster in a dense than in a less dense medium. However, Descartes had himself justified this assumption by saying that something with the propensity to motion may be subjected to its laws although not itself undergoing finite motion.

These two points are easily dealt with. The apparent overstatement of Kepler's achievements arose out of a mistranslation of Browne's. The question of whether Descartes had satisfactorily justified his treatment of light as a finite speed when he normally

¹¹¹ Op cit(109) 2368.

considered speed as infinite, could only be a matter of opinion.

Certainly, Gregory could have pointed out that Descartes had attempted to justify his position, but, if he did not accept the justification, there was no need for him to do so.

The four occasions on which Gregory is found to have invoked physical causes for the behaviour of light, after his resolve not to do so, are interesting. Similar comments are present in the Edinburgh lectures and in other writings of Gregory's. All the comments cited (and these four are the only ones in the book) rely on some form of the least time principle, summed up as 'Nature's method of operating is the easiest and most expedite'. This concept underlay much of Gregory's work and is discussed more fully in the Conclusion to this thesis. For the moment we may remark that he had Newton's endorsement, for it was from Newton that he received the fluxional version of Fermat's analysis.

When we look at the optical authors from which Gregory could have drawn these lectures - Kepler, Descartes, Barrow, Dechaies, Huygens, Boyle - we see how far he did avoid fruitless speculation. James Gregory's Optica Promota had adhered to a geometrical treatment, and this may have influenced David. Even Barrow, whose preface emphasized that the reader must lay no great trust in any physical premises he might lay down in his largely geometrical treatment was led to speculate on such topics as the nature of light. Seen in its context, Gregory's work is remarkably free of such theorizing.

Moreover, to him the least time principle was a mathematical rather than a physical principle. The Biographia did not object to Gregory's assumption that causes are proportional to their effects in

his treatment of refraction, but to Gregory, the least time principle was just as much a basic law. His use of it was not always very happy, but it was clearly very different from analogies between light corpuscles and balls bouncing on the ground or breaking through cloth - and it was such analogies as these which Gregory avoided as fruitless speculation. The least time principle was in many ways a universal, unifying principle which might be applied to all situations. It was certainly not as narrow as a physical assumption about the nature of light. (See Conclusion).

The criticisms in the Biographia, then, did not take due account of the age in which Gregory wrote. What seemed to that author a physical assumption on the nature of light was no such thing to Gregory, who saw his text, as indeed it was in his sense, as totally free of all such vain speculation.

2.7 Edinburgh Astronomy Lectures: 'Institutiones Astronomicae'

Gregory's papers on education had set down a plan for learning astronomy which is precisely what we find in Gregory's lecture course. Parts one to three of his astronomy lectures consider the true and then the apparent system and give examples of calculations based thereon. We have considerably more copies of these parts than of part four which was written some years later and dealt with planetary theory suggested by Gregory as an optional subject. Only one copy has survived of any extra studies, in the form of a set of lectures on 'Horolographia' or gnomonics.

For sources, Gregory especially recommended Kepler, but also suggested Mercator's Institutions as an introduction. For the theory of the planets he recommended Streete's Caroline Tables or Flamsteed's edition of Horrox's tables¹¹². In fact, there is no sign that Gregory used Kepler's works directly to prepare his lectures, for Mercator's work would have supplied the information he gives of that astronomer. Instead, the lectures are almost entirely based on Mercator's Institutions and frequently taken verbatim from it. Streete's tables were also used in part four. Others, such as Descartes, have some influence on the content of these lectures. The gnomonics notes may have come from any source.

¹¹² Nicolas Mercator Institutionum Astronomicorum ... (London 1676).

Thomas Streete Astronomia Carolina, a new theorie of the coelestial motions ... (London 1661).

John Flamsteed's edition of Horrox's tables was included in his 'Doctrine of the sphere' in Jonas Moore A new systeme of the mathematicks ... 2 vols (London 1681).

The first three parts were given in 1685 and 1686, while part four was not written until 1689. (This at least is the date on Gregory's autograph copy, and I have found no earlier student copies.) The gnomonics were probably given after part four, but they are undated and may have been given at any time. The time lag meant that, unlike the first three parts, part four was written after Gregory had read the Principia, and it shows some differences in sources and in attitudes. As such, it is convenient to discuss it separately. Similarly, the gnomonics lectures are of an entirely different style, and will be discussed on their own.

Kepler's laws are discussed here, and, more fully, in 5.3.1. Briefly, the first law states that planets move in an ellipse with the sun at one focus. The second states that the radius vector attaching the planet to the sun sweeps out equal areas in equal times. The third states that the squares of the periodic times of the planets' orbits are as the cubes of their mean distances from the sun. It was the second law which gave rise to 'Kepler's Problem', discussed in 3.3.3.

2.7.1 Parts one-three: content and sources

Part one begins with a general description of the universe as an infinite space of vortices, and then describes more particularly the motions of heavenly bodies. The paths of planets and their satellites, the sun and the stars, and of comets, all with regard to a heliocentric universe are described. The effects of the annual and diurnal motions of the earth are given due consideration.

Part two considers the doctrine of the sphere, in which the stars are regarded as points on a sphere, which is divided by various sets of

circles arising out of the earth's motion, or of the position of the observer. Examples of the first would be the ecliptic and equinoctial circles, and, of the second, the horizon. Gregory defines the zodiac, describes the constellations and explains how to define the position of a phenomenon in terms of these systems of circles. Finally he gives a qualitative discussion of refraction and parallax, and, in some copies, a discourse on the vanity of astrology.

Part three is divided into two sections, both giving examples of worked problems. The first section considers the use of the celestial and terrestrial globes, and the second considers the apparent motions of the stars.

The very title of these lectures, Institutiones Astronomicae, echoes Mercator's work and we find first that much of the lectures is taken word for word from Mercator, and secondly that there is very little in them which cannot be found in Mercator. For example, chapter 5 of part two 'On the Division and Parts of Time' is copied almost verbatim from Mercator, as are the problems which make up the whole of part three¹¹³. The physical constants which Gregory uses to measure magnitudes and distances in the heavens are taken from Mercator, especially from his Appendix which gives many recent observations. Thus, although Gregory refers to the observations of Cassini or to those in Huygens' Systema Saturnium, he has probably taken these references at second-hand from Mercator's appendix.

However, Gregory does in some instances use material which is not in Mercator, or present that material in a different way. His

¹¹³ Mercator op cit (112) bk 1 chap 2 22-6, chaps 3, 4 26-61

introduction, which sets out the Copernican universe is perhaps the clearest example of a different presentation. Since he determined to explain this to his students first, before considering the apparent system, Mercator's work, which follows the contrary approach was not relevant. Certainly, a thorough knowledge of Mercator was quite sufficient for the material Gregory uses here, but he represents it in a totally different framework. Moreover, to illustrate the apparent system, Gregory uses the device of comparative astronomy, which he was to extend into a complete book of his Astronomiae. That is, he encouraged his students to imagine how the world would appear to an inhabitant of another planet, thus helping them to make the conceptual change from real to apparent system. The technique was not altogether new; Kepler's Somnium, for example, had considered the appearance of the world to an observer on the moon. However, Gregory's treatment seems to have been more extended than he could have found in previous work, and it is skilfully handled.

Mercator had discussed the main systems of planetary theory and had set out Kepler's laws. Gregory's treatment of these laws is discussed in detail in Chapter 5, but here, too, he differs from Mercator, who, although admitting the difficulty of applying the area law, accepted all three as rules of nature.

Gregory states the third law in part one of his lectures and refers his students to Kepler for more on the harmony of the universe. However, although he certainly accepted the first law when he wrote part four, even then he did not mention the second one. These first two laws are not mentioned in parts one-three of the lectures, where Gregory even refers to the circular path of the earth centred on the

sun. He draws this path as an oval in one figure, but this is the result of projecting the Copernican sphere onto a plane and does not represent an actual elliptical path.

This change in attitude between the first and fourth parts of Gregory's astronomy lectures may simply reflect his conception of the needs of his students. Unless they are actually going to study the planetary motions themselves, it makes little difference whether the orbits are elliptical or circular. Only when he is introducing planetary theory does it become necessary to introduce the refinement of elliptical orbits.

However, in that case he might as well have posited elliptical motion in the first place. That he did not seems more likely to have been the result of the influence of Descartes. The Cartesian vortex theory, although it allowed the planets to have orbits which departed somewhat from perfect circles, nevertheless was not adapted to Kepler's laws; most obviously, the sun was placed in the centre of the vortex. In part four of the lectures Gregory had read Newton's Principia and abandoned Descartes' vortex theory, but parts one - three were set in just this system.

The opening lines of the lectures posit a world in which each star is a sun and, like our own, the centre of a vortex in which its planets are carried round. This assumption is maintained throughout the early parts of the lectures. Of course, the source of the concept may have been in another Cartesian author rather than Descartes himself, but as we know Gregory had used Descartes' Principia the previous year for the definition of motion in his mechanics lectures (see 2.8.1), it is unnecessary to posit another source.

Cartesian philosophy began to enter the curricula of Scottish universities in the 1660's, and, although some continued to fear its atheistic implications, it was generally praised in the dictates of the 1670's and 1680's. For example, Andrew Massie's dictates on natural philosophy given at Edinburgh in 1682, three years before Gregory's astronomy lectures, are full of praise for Descartes and his new philosophy. Kennedy's lectures at Edinburgh in the 1680's also accept Descartes' ideas, though they are somewhat more critical than Massie's¹¹⁴.

Thus Gregory's choice of a Cartesian framework in which to set Mercator's practical work on the motions of the heavens is not surprising. Avoiding questions such as the existence of a vacuum, on which there was dissension, Gregory was giving the cosmological scheme with which most of the Scottish regents of his day would have agreed.

In one case, at least, we find evidence of a lack of broad knowledge of modern developments. This is in the value he takes for the earth's radius. The rough and ready value of a meridian degree used by seamen was ^{statute} 60 miles, but this had been improved throughout the seventeenth century. Richard Norwood's value of $69\frac{1}{2}$ miles had appeared in 1637¹¹⁵. Furthermore, Jean Picard's highly accurate measurement of 57,060 toises had not only been published in France, but had also appeared in the Transactions for 1675¹¹⁶. However, Gregory's astronomy lectures give only a measure of 3,440 miles for the Earth's radius,

¹¹⁴ Shepherd op cit (12) ii, 217-8, 219.

¹¹⁵ Richard Norwood The seaman's practice (London 1637)

¹¹⁶ Jean Picard Mesure de la Terre (Paris 1671) and PT 9 (March 1675) no. 112 261-72.

corresponding to the old value of 60 miles for a meridian degree. Certainly, Gregory had seen some copies of the Transactions; they are used, for example, in his optical lectures. Equally certainly, however, even if all of them were available to him in Edinburgh, which is extremely doubtful, he did not have a thorough knowledge of their contents.

However, in three instances, these astronomy lectures show a knowledge beyond Mercator and Descartes. These instances are Cassini's observations of comets, Hooke's and Cassini's observations *misinterpreted as a parallax effect* of ~~parallax~~ and his comments on astrology.

Basically, Gregory agreed with Descartes about comets - at least in so far as they were passed from vortex to vortex and unlikely ever to return. However, Cassini's theory of the motion of the comet of 1664 had suggested that comets might travel in closed orbits, and Gregory mentions this theory¹¹⁷.

He also discusses the problem of stellar parallax; if the earth is truly moving through space we should observe shifts in the relative positions of the stars, and they should appear to approach and recede from us. At least, as the earth travels with its axis parallel to a fixed direction, it should seem to point at different stars at different times of the year. We do not generally perceive these things, because the distance of even the nearest star is so immense when compared with the diameter of the earth's orbit. However, they may be detected with very accurate instruments and Hooke and Cassini have done so.

¹¹⁷ René Taton 'Gian Domenico Cassini' DSB 111 100-4 p101.

While we have no clue as to the source of Gregory's knowledge of Cassini's theories of comets, it appears likely that his knowledge of the observations of parallax purportedly made by Hooke and Cassini came from his uncle's papers. Robert Hooke's tract 'On the Motion of the Earth' contained his observations of stars near the zenith, by which he attempted to prove the earth's motion¹¹⁸. James Gregorie, having seen this work, was prompted to write to Oldenburg with an account of his proposal for measuring parallax¹¹⁹. Also a copy of the method was sent to James Frazer in Paris, who passed a copy of it on to several in the capital, including Cassini. With his reply to Gregorie he sent a copy of Cassini's comments on the method¹²⁰. These comments are now unfortunately lost, but they probably came to David on his uncle's death and, if Cassini was himself then making observations of stellar parallax, he must have mentioned them. Thus David would probably have found both Hooke's tract and at least some mention of Cassini's observations among his uncle's papers. Perhaps he knew of Cassini's theories of comets from the same source.

Gregory's easy acceptance of the validity of these results contrasts strongly with his suspicious attitude over 15 years later to Flamsteed's very similar observations. In his Astronomiae he heavily criticised these observations and put forward the claim of his uncle's method as the best way of measuring parallax. His treatment of Hooke's observations in the astronomy lectures, where his uncle's method is not

An attempt to prove
¹¹⁸ Robert Hooke/~~On~~ the motion of the Earth (London 1674).

¹¹⁹ Gregorie to Oldenburg 8.6.1675 GTU 306.

¹²⁰ Frazer to Gregorie 10.8.1675 GTU 323.

mentioned supports the arguments of Chapter 5 that Gregory's rejection of Flamsteed's observations was based on personal rather than scientific views.

Finally, part two of the lectures sometimes ended with a chapter on astrology which is certainly not derived from Mercator. It does not appear in Gregory's original of 1685, but it is in the amanuensis copy in Christchurch, which was corrected by him. It is in two student copies of part two, dated 1690 and 1693, but not in two others, one dated 1705 and the other undated¹²¹.

This chapter roundly condemns astrology, which is built on unsound principles and used by unscrupulous men to gain their own ends. It might even then be supportable were it used only in petty ways to dupe the common people. However, astrologers also cast horoscopes of kings and princes and derive their life, government, character and death from these false principles. Those best of kings, Charles I and II were both harmed in this way, for seditious men would not have been able to persuade the people to take up arms against them had astrologers not urged that this was a time at which the monarchy might be easily overthrown. Astrology is rightly banned by the Church and as good men and as Christians we should ban it from astronomy.

Other seventeenth century authors had written in a similar vein. For example, Dechaies' Cursus devoted a section to an extended discussion of astrology and, after examining its principles, rejected them as unsound¹²². Henry Savile, when he drew up the statutes for his

¹²¹ EUL MSS La 111 570, Dc 612, DC 67 69, AUL MS 2171

¹²² Dechaies op cit (69)iii 636-60.

Savilian professors included a clause stating that his astronomy professor was

'utterly debarred from professing the doctrine of nativities and all judicial astrology without exception'¹²³.

Perhaps this clause may have had something to do with the omission of the astrology chapter from some copies of the lectures, but there is no evidence of such a connection.

Yet belief in astrology was still wide-spread. John Evelyn, an early member of the Royal Society, expressed his own opinion of these 'knavish and ignorant stargazers' when he recorded the solar eclipse of 29th March 1652. This event

'so much threatened by the Astrologers ... had so exceedingly alarm'd the whole Nation, so as hardly any would worke, none stir out of their houses'¹²⁴.

Evelyn not only ridiculed astrology, but was also a staunch Royalist, yet he makes no mention of astrological predictions which aided the Parliamentary side in the Civil War, such as Gregory refers to. He attributed unseasonable weather in January 1662 to God's anger against the British for murdering their king, but it was hardly this sort of phenomenon which Gregory meant¹²⁵.

¹²³ Ward op cit (8) 274.

¹²⁴ The diary of John Evelyn edited by E.S. de Beer (London 1959) 319.

¹²⁵ Ibid 434.

The only astrological sign connected with the Civil War which I have been able to trace was the meteor which appeared over Edinburgh before the battle of Dunbar in 1650. This meteor pointed towards England, and was taken by the Covenanting forces as a most encouraging sign¹²⁶. They may have used this portent to help in raising armies for their side. However, the Covenanters were at this point fighting for Charles against Cromwell, and in any case lost the ensuing battle, so it is unlikely that this was the event to which Gregory referred.

This chapter is also interesting because it is virtually the only occasion on which Gregory made a public statement of his Jacobite sympathies. In general, he was careful to preserve a non-committal public attitude over politics and religion and the expressions of sympathy with the Stewart kings suggest that these astrology lectures were read before 1688. At the instigation of the committee of visitation, lecture notes were examined in 1690 for signs of dissatisfaction with the government or Presbyterian church¹²⁷. These remarks might have been thought enough to signify dissatisfaction with either the Hanoverian regime or the Scottish church, and perhaps Gregory, having included these comments when he gave the lectures in 1685, took care to remove them from his own copy and as many student copies as he could before the committee examined his lectures. Then, in the comparative freedom of Oxford, he may have felt that it was safe to put them back into the copy he had made of these Edinburgh

¹²⁶ G. Holden Pike Oliver Cromwell and his times (London 1899) 203.

¹²⁷ B26.

lectures.

The first three parts of Gregory's lectures on astronomy, then, present Mercator's Institutions set in a Cartesian framework. Some differences in presentation are found, and his comments on comets, parallax and astrology did not all come from Mercator. This use of the Institutions, though we may criticize Gregory for never mentioning his source, provided his students with a competent introduction to astronomy, which his presentation helped to clarify.

2.7.2 Part 4: Content and sources

Gregory's introduction to part four clearly sets out his aims. He will not attempt to set down everything that has been written on planetary theory, for these things may be found among the writers on astronomy. Rather, he intends to show his pupils how to solve problems involving the determination of lines and angles which place a phenomenon in the heavens. Such problems are the basis of tables of planetary motions, and he will show how these tables are constructed and used.

Thus, the fourth part sets out relevant definitions and examines such problems as finding the obliquity of the ecliptic, or defining the orbit of a planet from certain observations. Many of these are taken directly from Mercator, or only minimally recast. Others are drawn from Streete's Astronomia Carolina, whose tables are frequently referred to.

Unlike part one, Gregory here states that the planetary orbits are ellipses. There is little doubt, he says, that the planets travel in ellipses about the sun, as their satellites do about them. It is not so certain whether the sun is in one focus of such ellipses, or

whether the other focus is a centre of mean motion (seen from which, the planets travel with a uniform speed). Gregory admits that these suppositions might be contrary to demonstration, but they are supposed by writers in astronomy, in developing the theory of the planets, so he will suppose them now. Kepler's second law is still not mentioned.

In this chapter, Gregory uses the equant devices of Ward, Bouilleau and Streete (which retain the elliptical orbit of the planets, but are strictly contrary to Kepler's second law, although easier to apply in practice). In fact, Streete had used Bouilleau's method, which was ^{similar to} ~~a refinement of~~ Ward's and Gregory gives us both these methods. Bouilleau's is to be used only when Ward's proves too inaccurate, which is especially likely to happen in the case of Mars, whose eccentricity is greatest. He refers his students to page 42 of Ward's Astronomia Geometrica¹²⁸, for the determination of a planetary orbit from 5 given centric positions. It seems that Gregory had studied Ward's own work, and not merely learnt of it through reading Mercator.

When Gregory comes to discuss the theory of the moon, he at last introduces Newton's work. There is yet no natural and probable theory of the moon, he warns his readers. However, this statement excepts Newton's theory, for which no tables have yet been constructed - though no doubt they soon will be constructed. Hitherto astronomers have concerned themselves with theories which satisfy the appearances of the moon, rather than her nature; that is, theories which save the phenomena, but do not explain them. By implication, Newton's theory explains as well as approximates the moon's motions.

128 Seth Ward Astronomia geometrica ... (London 1656).

However, Gregory's aim is reiterated; as with the planets, he wishes only to explain how to use the existing tables to calculate the moon's positions. Other tables might be more accurate than Streete's, but since these have been used so far for the planets they will be used also for the moon, as it is hardly worthwhile to undertake the explanation of a new system now. Thus the subjoined problems on calculating the moon's orbit and her position in it are taken from Streete and Mercator, as were the problems on the planets.

In his conclusion, Gregory lists the many other matters which he might have discussed. More might have been said of eclipses, such as the method of showing them in a chart, or de la Hire's use of spherical triangles to compute them. The periodic appearance and disappearance of some fixed stars might be calculated. Huygens Systema Saturnium which deserves the attention of all philosophers, discusses the phenomena of the ring and satellites of Saturn. There are also the bands of Mars, and Jupiter's four satellites, of whose motions Flamsteed has devised tables and a model. We might calculate the stationary points of the planets and the aspects between them, or find the sun's period of rotation from the motion of the sunspots. There is the latitude and longitude of the moon, which we could determine for different meridians on earth. (This was one of several methods suggested for tackling the problem of finding longitude at sea). The moon's motion can also be determined. Newton has devised an elegant hypothesis to account for the moon's librations. (Gregory's knowledge of this hypothesis does not evidence any reading in the Principia. Newton's theory had been published by Mercator, with due acknowledgement in the appendix to his Institutions). Many

more phenomena in the heavens may be calculated. Especially, since we now know that a finite time is taken by light in crossing the heavens, we could calculate the differences between the true and observed times of phenomena. However, the dilligent student should now be able to handle such topics from the Alphonsine, Prutenic or Rudolphine tables, provided he studied carefully the instructions for their use given in these tables.

This conclusion shows that Gregory's study had gone beyond Streete, Mercator and Ward. Mercator's appendix discussed many of these topics; besides Newton's theory of lunar libration, it mentions sunspots, Huygen's work on Saturn, comets and variations among the fixed stars. However, it did not discuss de la Hire's methods, nor Flamsteed's theory of Jupiter's satelllites, which was contained in the Transactions for 1673¹²⁹. The Transactions had also contained Roemer's discovery of the speed of light deduced from observations of Jupiter's satelllites¹³⁰. However, Newton's Principia also gave this result, mentioning that light would take 10 minutes to pass from sun to earth, a point which Roemer did not make but which Gregory repeats, suggesting that this is another example of his study of the Principia¹³¹.

In the main, though, part four of the astronomy lectures was taken from Mercator, with Streete for further details and examples. The Transactions, the Principia, de la Hire's tables, Huygens Systema

129 PT 8 (July 1673) no. 96 6094-7000.

130 PT 11 (June 1677) no. 136 893-4.

131 Newton op cit (89) bk 1, scholium to prop 96 p.231.

Saturnium and the others which Gregory mentions may have been familiar ground to him, but he did not make any extensive use of them in his attempts to give his students a grounding in planetary theory.

Like parts one to three, these lectures are not original: but originality was not their aim. They are competent accounts of the celestial phenomena, which make an intelligent use of sources by presenting material in an orderly way accompanied by lucid explanations. They explain only the minimum necessary to achieve their goal; the production of students capable of plotting the paths of the heavens. The omission of Kepler's second law, and of Newtonian gravitation theories does nothing to detract from this goal. Their inclusion would not have helped to bring it closer. In short, if we accept the end to which these lectures were written, the only criticism we can make is that Gregory's major source, Mercator's Institutions is not mentioned once in the course of the lectures.

2.7.3 Gnomonics

Gnomonics, horolographia or dialling is the art of drawing sun moon or star dials onto any surface, generally a plane. We have one set of lecture notes on this topic attributed to David Gregory, in a notebook entitled 'Systema mathematica authore D. Gregory'. However, this notebook also contains lectures on hydrostatics, and there is some doubt as to whether these were the work of David or of his brother James. If we are doubtful of the authorship of these lectures, we must therefore be doubtful of the authorship of the gnomonics lectures also.

However, if the lectures were not given by David, they were at least given by someone who knew his standard lecture course.

Propositions in both the *Geometria Practica* and the astronomy lectures are referred to here. James though, and possibly some others, would certainly have been able to make such references.

If David did give them he must (unless he gave them at Oxford which there is no reason to suppose) have read them at the end of the session 1689-90 or, more likely, in the session 1690-91, for they refer to part four of the astronomy lectures, which was only read in 1689-90. These times were difficult ones for Gregory. Arguments over the visitation committee persisted, and although Gregory began his lecture course in December 1689, this was without the approval of the Town Council. Andrew Massie, for one, seems to have done all he could to sabotage his lectures. Under these circumstances, we should not be surprised if only one copy of Gregory's lectures on gnomonics has survived.

Moreover, the lectures begin by warning that the intending student should have a knowledge of the elements of geometry and astronomy. Spherical trigonometry is also necessary for this study. Clearly, this was one of David's most advanced courses, and few would have reached it, so that again we would not expect to find many copies.

Clavius and Dechaes were the authors David recommended for this topic, and either could have supplied the basis for these lectures. Ten propositions give the principles of this science in a clear and concise course. These lectures form a competent appendix to the '*Institutiones Astronomicae*'.

2.8 Lectures on Mechanics: 'Geometria de Motu'

If Gregory showed more originality in other courses, such as his optics lectures, it was in these mechanics lectures that he displayed his familiarity with the widest variety of sources. The four works most used were Wallis' De Motu, Dechalet's Cursus, Torricelli's De Motu Gravium and Huygens' Horologium Oscillatorium¹³². However, Gregory referred not only to Huygens' experiments, but also to those of Mariotte (citing his Traité de Percussion ou Choc des Corps), Galileo, Mersenne and Riccioli. The experiments of these last three were mentioned by Dechalet, but Gregory appears to have known more, at least of Mersenne's work, than he could have learnt from that source. Descartes' Principia provided his definition of motion and may have influenced him in the importance he gave to the question of impact. The treatment of impact, however, is certainly not Cartesian, and makes it probable that Gregory had seen the papers of Wallis, Wren and Huygens in the Transactions on this topic. Gregory's also gives Catalan's defence of Descartes against Leibniz's attack on his statements about the conservation of quantity of motion. (This defence was published after Gregory's death

¹³² John Wallis Mechanica; sive de motu tractatus geometricus (London 1670-71). Page references are to its later publication in Wallis Opera Mathematica 3 vols (Oxford 1693-99) i 573-1063.

Dechalet op cit (69)

Evangelista Torricelli De motu gravium naturaliter descendentium et projectorum libri duo. The copy I have used is bound with his De sphaera et solidis sphaeralibus libri duo (Florence 1644) 95-243.

Christian Huygens Horologium oscillatorium, sive de moto pendulorum ad horologia aptato demonstrationes geometricae (Parisiis 1673).

as if it had been his own). For the effects of air-resistance on the motion of projectiles, Gregory could refer to James Gregorie's Tentamina Geometrica de motu penduli et projectorum. Finally, Newton's Principia was used in the final part of the lectures.

Gregory frequently copied propositions, and even proofs word for word from these sources. Moreover, although Huygens is mentioned in part 5, and Torricelli's development of the military square is attributed to him, neither of the other major sources is mentioned even once. Thus, without an acquaintance with these sources, Gregory's mechanics lectures would give a quite inflated view of his abilities.

Nevertheless, the range of sources indicates at once a wide study in the field. Huygens' Horologium was a particularly advanced and difficult work. Yet Gregory handles these authors with confidence, and can generalise a proposition or adapt terminology. Aimed towards practical ends, these lectures served their purpose. They explained the basic principles of motion and impact, the use of simple machines, the behaviour of projectiles (and so how to aim canon) the behaviour of pendula (and so the mechanism of pendulum clocks).

The works used bear no especial relation to those mentioned by Gregory in his recommended texts for the study of geometrical physics, except that both sets include Torricelli. However, the course does follow that suggested for mechanics. The principles of motion and the five simple machines are contained in the beginning of part one and in part three. These were the basic studies which Gregory proposed and appear to have been the most popular parts of the lectures. The optional topics - impact, descent under gravity,

ballistics, pendula and hydrostatics - are all found here. All but hydrostatics have their own place in the five part course of mechanics lectures which is examined below. Only hydrostatics was delivered as a lecture course on its own, and as we are unsure of its authorship and as it displays certain individual features we shall discuss it separately.

The lectures were all given between 1684 and 1688; parts one and two in 1684-85, part three in 1685-86, part four in 1686-87 and part five in 1687-88. Thus only part five was given after Gregory had seen Newton's Principia. An interesting feature of the copies of these lectures is that two of them summarize the lectures, and relate them to the appropriate parts of Wallis' De Motu. One, in Christ Church, is of parts one to five, and the other, in St. Andrews and probably the copy of David's nephew and namesake (see 2.3.4) is of parts one and two only¹³³. This summarized copy is the only one, apart from Gregory's original, of part five, on pendula.

Broadly, we can correlate the five parts with our four major sources as follows: part one is drawn from Wallis, part two from Wallis and Dechaies, part three from Wallis, part four chapter 1 from Huygens and Wallis and chapter 2 from Torricelli and part five from Dechaies, Huygens and Wallis.

I have examined each part of the lectures in turn below, subdividing part one, which is untitled, into general principles and laws of impact.

¹³³ Ch Ch MS 131 SUL MS QA35 G8L4.

2.8.1 Part one General Principles

This section consists of nine definitions, five axioms and four propositions which set out the basic properties of uniform motion. On two occasions it was prefixed, with only the additional definitions examined in part 2.8.2, to part three on simple machines.

Most of the definitions, all the axioms and the first two propositions are taken from Wallis. Some of the definitions are slightly altered to lay greater stress on either their quantifiable or their descriptive aspects, but only the definition of motion and the treatment of momentum differ in essentials.

The definitions of space traversed by a moving body, speed, equal, greater or lesser speed, direction of motion and impediment are all taken directly from Wallis. The definitions have at times a scholastic ring; speed, for example is 'an affection of motion'. However, if we read the definition in full, we find

'speed, or velocity [Gregory has added seu velocitas to Wallis' celeritas] is an affection of motion, which determines how much space is crossed in any time'.¹³⁴

We may see this definition in two halves; a descriptive phrase which echoes scholastic definitions, and a relation of speed to two other quantities, space and time, by which it may be quantified.

When Gregory defined time, he amplified Wallis' simple definition as 'the space of time in which a motion is carried out' with a descriptive introduction:

¹³⁴ Gregory 'De Motu' def.4; Wallis op cit(127) def. IX p.576.

'Time is the continuance of anything in its being.

But everyone knows that some things remain in being longer than others, have been when they are not, are when they have not yet begun, to define a series, and some begin and end together with others. But when we use the word time ordinarily we understand that space of time in which a motion is carried out'¹³⁵.

Thus, the idea of time as marking a sequence of events is prefaced to Wallis' simpler concept of time as defined by motion.

On the other hand, Wallis' definition of motion was too imprecise for Gregory. Wallis had said only 'By motion, we understand local motion'¹³⁶, which is the Aristotelian way of distinguishing ^{change of place} ~~'violent' from 'natural' motion~~, that is, from such phenomena as generation and corruption. Instead of adopting this definition, Gregory had explicitly adapted Descartes' definition

'We can say that [motion] is the translation of one part of matter, or of one body, from the neighbourhood of those bodies which immediately touch it; and are considered as at rest, into the neighbourhood of others'¹³⁷.

A body, says Gregory in his first definition, moves if the distance

¹³⁵ Ibid def. 2; def. VI p.576.

¹³⁶ Wallis op cit (127) def. 11 p.575.

¹³⁷ René Descartes Principia Philosophiae (Amstelodami 1644) pt. 2 para. 25.

between it and any of three other bodies said to be at rest, and not lying in the same straight line, is changed. This, he points out, differs from Descartes' definition only in that the Frenchman's is 'ad stylum philosophicum', while his 'ad geometriam magis accomodetur'. However, probably because none of his sources do so, Gregory makes no further use of this definition.

Clearly, Gregory was aware of the need to quantify concepts - and those lectures were written three years before he read the Principia. Newton's quantification was to be much more successful than that of Wallis and Gregory, but he was certainly not the first to see its necessity.

After these definitions, Gregory sets out his axioms. Wallis had followed his definitions with six propositions on the composition of ratios which Gregory assumes. However, Wallis' following propositions 7 - 11 become Gregory's axioms 1 - 5. The first of these was the statement that effects are proportional to their causes, a principle which Gregory was to use several times in other contexts¹³⁸. Secondly, the aggregate of opposites is their difference and of agreeable quantities [congruentium] their sum. Thirdly, if one of two equal quantities is increased, or the other decreased, the first will be greater. Fourthly, the aggregate of momentum and impediment should be found, and will be of the same sort as whichever of these two is greater. Fifthly, if momentum exceeds impediment motion will be begun or increased, or if impediment is greater, motion will be

¹³⁸ In, for example, his *Optics*, where he deduces his 'Keplerian' alternative to the Cartesian explanations of reflection and refraction.

halted or decreased. If both are equal there will be no change in the state of rest or motion.

Clearly, such statements are more truly axiomatic than susceptible to proof (in the context of seventeenth century mathematics, at least).

The propositions that follow constitute what we would consider definitions of speed and momentum. Following Wallis' propositions 23 - 25, Gregory shows first that speed is proportional to distance covered in a given time, then that it is inversely proportional to the time in which a given distance is covered. Hence speed is as distance divided by time. Gregory's proofs lack the algebraic notation of Wallis', and here we must remember the lack of mathematical sophistication among Gregory's pupils. Otherwise he follows Wallis' proof structure. Of course, such statements are not capable of any rigorous proof, and their demonstrations rely heavily on an intuitive understanding of the principles involved. Wallis, and so Gregory, merely states that at double the speed, double the distance will be covered, at half the speed, half the distance and so on. Thus speed is proportional to distance. Wallis appeals to his definition of speed (which he said results from a comparison of length and speed, a point omitted by Gregory), but Gregory does not make even this appeal.

It is clear from this treatment of what appears to us the simple matter of defining speed as distance over time how far Wallis and Gregory were from a modern treatment. They were aware of a need to quantify concepts, and allowed for this in their definitions, but they left the quantification itself to propositions. Newton might define quantity of matter as the product of density and magnitude, and

quantity of motion as the product of velocity and quantity of matter, but apparently Wallis and Gregory could not take this step.

In the following propositions, Gregory discussed momentum, but before we understand his approach we must see how Wallis treated momentum and related concepts, and how Gregory defined the concept.

Both Wallis and Gregory gave similar definitions of momentum, both of the descriptive, scholastic type. Wallis had said

'Momentum, apello, id quod motui efficiendo conducit'¹³⁹.

Gregory altered this to

'Momentum, seu quantitas motus est potentia in corpore
producendi motum tot taliumque effectuum'¹⁴⁰.

That is, Wallis called momentum 'that which leads to effecting motion', while Gregory called it 'the potential in a body of producing motion of such and so great effects'. The restriction in Gregory's definition to 'in a body' was to prove significant, but the 'tot taliumque effectuum' might be seen as either an attempt to suggest the direction which quantification might take, or as a scholastic flourish.

Force, which Gregory did not define, Wallis described thus:

'Vim motricem, vel etiam vim simpliciter, apello
potentiam efficiendo motum'¹⁴¹.

That is, motive force, or simply force, was the 'potential of effecting motion'. Like Gregory's momentum, this force is the potential through

¹³⁹ Wallis op cit(127) def. 111 p.576.

¹⁴⁰ Gregory 'De Motu' def. 8.

¹⁴¹ Wallis op cit(127) def. V p.576.

which motion is created, and, looking at the definitions alone, Gregory's momentum seems to fall between Wallis' momentum and his force.

A paragraph below his definition of momentum, Wallis had added 'To momentum I refer motive force and time; by as much as these are greater, so much more is motion effected'.

In proposition 20 he developed this further (in a similar manner to his propositions on speed) and showed that momentum is the product of force and time. This concept was opposed to that of impediment which was the product of weight and distance. Westfall has analysed the way in which these concepts, and Wallis' ideas on force arose from his study of the lever as a primary mechanical model¹⁴².

Further in Chapter 3, 'momentum' was used as the moment of a force, or the product of force and distance. In this case, however, the equivalent term 'ponderatio' is introduced at once, and used thereafter. In the paper which he submitted to the Royal Society on impact, Wallis had used both 'vis' and 'impetus' to express the quantity, weight \times speed¹⁴³. However, in Chapter XI of the De Motu he used the term 'momentum' for this quantity. In the demonstration of his first proposition here he says that the 'Momentum seu Vis' of a body weighing mP , moving at a speed rC is $mrPC$. Subsequently in this chapter, and in Chapter XIII on elastic impact, he used momentum in this sense without specific definition, and without the

¹⁴² Richard Westfall Force in Newton's Physics (London 1971) 235-7.

¹⁴³ PT 3 (Jan. 1669) no. 43 864-6.

qualifying 'vis'.

Meanwhile, the term 'vis' was used in several different senses. In particular it was used in our modern sense of force and put proportional to acceleration, it was used as in the example above for momentum, or it was used for the change in momentum Δmv . On occasion, it was also implicit in the product of a force and the displacement of its point of application, our work. These definitions were frequently used together and apparently interchangeably¹⁴⁴.

When Gregory used the De Motu for his mechanics lectures, he apparently saw his way through some, at least, of this confusion. His answer to the problem of 'vis' was to ignore it - he did not define the term, but instead avoided using it as far as possible. In his treatment of simple machines, for example, he substituted the term 'potentia', defined somewhat vaguely as whatever produces in something a tendency to move (see 2.8.4). Gravity, previously defined with Wallis as 'vis motrix deorsum', he then included as an example of a 'potentia'. He made no attempt to quantify this concept.

On 'momentum', however, Gregory was clear. This was the product of mass and speed (and his concept of mass was a little clearer than Wallis'). Propositions 4 to 6 of Gregory's lectures established 'momentum' as this quantity, in just the same way as he had previously analysed speed. In the same way, too, Wallis had derived 'momentum' as the product of 'vis' and time. Gregory was not altogether consistent in this use, and was to use it on occasion in part 2 for the virtual weight of a body on an inclined plane. However, this may be seen as a

¹⁴⁴ Westfall op cit (142) 239.

temporary lapse, and in general his use of momentum was restricted to the product of mass and speed. (In practice, since he considered the direction of motion, he was using mass \times velocity).

It may have been his intention to cover impact as the next topic which led him to choose this definition of momentum, but the important point is rather his recognition that a choice must be made. He had no solution to the confusion surrounding Wallis' 'vis', but even here he saw the confusion and could resolve to avoid it. In the discussion of momentum, Gregory saw the contradiction in Wallis' use of the term, and resolved it by selecting one definition (which was not Wallis' primary one) and using 'momentum' in that sense alone.

Unfortunately, Gregory missed one implication of his choice. Wallis' proposition 11, which became Gregory's axiom 5, states that if momentum overcomes impediment ('that which obstructs or impedes motion') motion will be begun or increased, with similar statements for the other cases. Wallis extended this later to deduce equilibrium conditions which are meaningless in terms of Gregory's sense of 'momentum'. As it stands, this axiom in Gregory's notes introduces a further complication to the concept of momentum, and is far less general than the statement in Wallis. Similar arguments apply to axiom four on the addition of momentum and impediment, which had been Wallis' proposition 10.

Gregory makes no appeal to these last two axioms, although the first three are used, implicitly for the most part, throughout the lectures. The two connecting momentum and impediment are simply forgotten, and we might have expected Gregory to have foreseen this and omitted them.

Otherwise, Gregory's treatment of the fundamental concepts of motion is exemplary. He uses Wallis as a basis, but does not follow him blindly. He sees where clarification of concepts is necessary and supplies it (even if this does involve omitting the concept 'vis' altogether). With the exception of these two axioms, Gregory gave his students a clear, unambiguous introduction to motion.

2.8.2 Part 1 : Impact

The study of impact was a vital part of Cartesian mechanics, and had received much attention in the seventeenth century. In a mechanistic universe, consisting only of particles in motion, impact is of crucial importance, and Descartes had attempted in his Principia to lay down laws for the behaviour of bodies on collision¹⁴⁵. Unfortunately, these laws, derived from the immutability of God rather than from experimental evidence did not correlate particularly well with experience. However, Descartes had made the study of impact of primary importance for those who studied his philosophy. Wallis relegated this topic to the final portion of his De Motu, but David Gregory, who followed Wallis' treatment, and so might have been expected to follow his arrangement, promoted it to first topic after the basic principles of motion. Here we seem to have yet another example of Gregory's interest at this time in the Cartesian philosophy. In the previous year he had used Cartesian arguments to establish laws of refraction and reflection and in the following year he would set his astronomy lectures in a Cartesian universe. Here we find him arranging his mechanics course according to Cartesian priorities.

¹⁴⁵ Descartes op cit (137) II 46-52.

However, if the impulse to this study was Cartesian, Gregory did not make the mistake of following Descartes' own laws. Three papers in the Transactions, submitted by Wallis, Wren and Huygens had given the correct laws for hard bodies, based by the latter two on the concept of a balance¹⁴⁶. Wallis' ideas had been expanded to include elastic bodies in Chapters XI and XIII of his De Motu and this was the major source from which Gregory drew his lectures.

First he gave a further 8 definitions: gravity, weight and mass, centre of gravity, direct impact and hard, soft or elastic bodies. Again, these were almost all Wallis' definitions; only the distinction between weight and mass and the definition of centre of gravity differ significantly. In particular, he adopted Wallis' definition of gravity as 'vis motrix deorsum', unable on this occasion to avoid the use of 'vis'. With Wallis, too, he declined to discuss the underlying physical causes of gravity. (This was in marked contrast to his Oxford lectures, where he devoted much time to this very question (see 2.13.2)).

Wallis had defined centre of gravity progressively. A plane of equilibrium cuts a solid into pieces of equal weight. An axis of equilibrium is a line such that every plane through it is a plane of equilibrium. Similarly, the centre of gravity is the point such that every line through it is an axis of equilibrium. Gregory defines it alternatively as the point about which a body may be rotated, retaining any position into which it is put. Of course, the two definitions are

¹⁴⁶ Wallis op cit(143) Wren PT 3 (Jan. 1669) no. 43 867-8; Huygens PT 3 (April 1669) no. 46 927-8.

equivalent as was well-known at the time. Dechaies, for example, gave both definitions, with the latter as a direct consequence of the former¹⁴⁷. A large portion of Wallis' De Motu was devoted to the determination of centres of gravity, a procedure which is based on his definition. That given by Gregory is (unless we derive Wallis' definition first) useless for such a task. However, Gregory did not intend to make such a study and so was free to choose the simpler definition which his students would be able to visualise more easily.

Gregory then adds to this

'A body is said to move as much as its centre of gravity moves, and in the same line, and in the same way to ascend or descend as much as its centre of gravity ascends or descends'¹⁴⁸.

This principle was not new, but it had not been given in Wallis' De Motu. Nor does Gregory make any use of it, and it is possible that, having taken his definition of a centre of gravity from some unknown source, he found this comment below it and added it to his lectures.

The distinction between 'pondus' (weight) and 'moles' (mass) is more interesting. Gregory had been careful to use only 'moles' in his discussion of momentum, and now he hints at the difference he understands between them. First he adopts Wallis' "'Pondus" is the measure of gravity'¹⁴⁹. He ignores, though, Wallis' attempt to draw

¹⁴⁷ Dechaies op cit (69) def. 2 p.489.

¹⁴⁸ Gregory 'De Motu' def. 13.

¹⁴⁹ Wallis op cit (132) def. 13 p.577.

a distinction 'si quod est' between 'pondus' which refers more to a balance and 'onus' which refers rather to a lever. Wallis went on to say that he would simply use 'pondus' in either case.

Gregory's definition seems to assume that an intuitive concept of the distinction is already in the minds of his listeners. He says

'Since in the following i.e. the study of impact between uniform bodies we consider bodies as homogeneous and so those which are of equal moles or magnitude have an equal measure of gravity, that is, are considered of equal weight, and those which are of unequal moles weigh in the ratio of their magnitudes, we shall sometimes use pondus in place of moles or magnitude'¹⁵⁰.

It is tempting to suggest here that homogeneous means of equal density and so the proportionality of mass and magnitude in this instance is a forerunner of the Newtonian definition of 'moles' as $\text{magnitude} \times \text{density}$. However, although this seems to be the concept which is emerging, it is very far as yet from Newton's formulation. On the other hand, we might suggest that 'moles' is simply a pseudonym for magnitude, but the use of 'moles' in Gregory's definition of momentum makes that highly improbable. Again, 'magnitudo' might be a concept conveying more than simply magnitude or size. The distinction between this 'moles' which has no clear, explicit definition and 'pondus', the measure of gravity, is not specifically discussed nor made plain. Nevertheless, a distinction is being made and the result is somewhat more than Wallis' 'si quod est'. Probably Gregory's ideas of this

¹⁵⁰ Gregory 'De Motu' def. 11.

distinction had arisen through his study of Huygens, whose terms he is using here.

There now follow 4 propositions on the impact of hard bodies, 11 on the magnitude of a blow, or the force of impact and 13 on the impact of elastic bodies, almost all of them from Wallis' De Motu. However, there is an important initial difference. Wallis gives rules for calculating motion after impact in various situations, but he does not explicitly state the principle of conservation of momentum. Huygens had done so in the paper he submitted to the Royal Society, and, once the principle is given, it is clear in Wallis' work. Gregory uses it throughout his discussion of the impact of hard bodies and derives his results directly from it. He makes, of course, due allowance for direction of motion, thus in effect considering momentum as 'mass \times velocity' rather than, as stated, 'mass \times speed'. No formal distinction is drawn between speed and velocity, but the distinction is made in practice. This use of the principle clarifies Wallis' statements somewhat.

Gregory explains in the scholium to proposition 10 that we must not expect exact agreement with experience of real bodies which are not perfectly hard. However, Mariotte's experiments on soft bodies, published in his French tract on the percussion of bodies, show the calculated results¹⁵¹.

Wallis defined the magnitude of a blow as the total change in momentum: that is, the momentum lost by one body plus that gained by

151 Edme Mariotte Traité de la percussion, ou choc des corps ... (Paris 1673).

another. Here Gregory follows him almost exactly, in both propositions and in their proof structures. He omits the proposition on the centre of forces with which Wallis completes this chapter, perhaps because it directly contradicts the statements of Huygens in his Horologium.

Gregory continues with Wallis' treatment of elastic impact, again following it throughout. With Wallis, he explicitly neglects any inquiry into the nature of the elastic force. Without naming his source, he adds some points from Huygens' analysis of impact. First he gives his analysis in terms of the centre of gravity of the two colliding bodies. He also notes, as Huygens had done, that the quantity of motion (and here he uses Huygens' term, instead of his own more usual momentum) is constant, provided we have due regard to direction. Further, he subjoins Huygens' statement that a body will receive a greater momentum from another if a third body, the mean of the first two, is interposed between them. Huygens had not proven this statement, and Gregory contents himself with an illustrative example, explaining that he omits a general proof because of its length¹⁵².

Two statements are included by Gregory that are not found explicitly in Wallis' De Motu nor in the communications of Wallis, Wren and Huygens to the Royal Society. First he notes that the relative speed of two elastic bodies is the same before and after impact, only the direction being changed. Secondly, if a body hits a row of equal ones, it will be halted and the body at the far end of the row will move

¹⁵² Gregory 'De Motu' prop. 31, cors. 1, 2.

on with the speed the first had before impact. However, the first of these is immediate from the alternative proofs which Wallis gives. The second is not a difficult result, and is immediate enough from everyday experience and the above results. We may remember, too, that Gregory was in London in 1681, when such topics were common knowledge.

Thus, part one is completed. Descartes is mentioned specifically for his definition of motion, and Cartesian influences appear in the importance Gregory gives to impact. Huygens' paper on impact was used, and Mariotte's experiments on the topic mentioned. Basically, however, granted a somewhat altered treatment of momentum, this is a restatement of Wallis' principles of motion and Wallis' laws of impact.

2.8.3 Part 2 : De Gravium Descensu et Motuum Declivitate

The second part of these lectures, like the first, is largely based on Wallis' De Motu. Indeed, it shares its title with chapter two of Wallis' work, from which most of it is taken. Even more than in part one, Gregory has precisely followed Wallis' treatment. His only departure from Wallis is his insertion of 5 propositions (out of 16) from Dechaes, whose Cursus covered similar ground as Book 3 of Staticae.

The approach here is dynamical. Beginning with the proposition that bodies gravitate in proportion to their weights, Wallis continues to find the proportion between the virtual weights of bodies on inclined planes or hanging freely. Gregory follows him through, occasionally making one of Wallis' propositions a corollary to that preceding, or minimally restating a proof, but making no important alterations.

However, when he used Dechaes' work, a complication arose over

the old problem of the term 'momentum'. Dechaies had used it in yet another sense, defining

'Momentum is the propensity to downwards motion'¹⁵³.

Thus, while Wallis (and Gregory) would say 'weights lying on inclined planes gravitate in the ratio ... ' Dechaies said 'the momenta of weights lying on inclined planes are in the ratio ... '. We would talk of the virtual weights of these bodies. Generally, Gregory was consistent in adapting Dechaies' terminology to Wallis, and so avoiding the introduction of another 'momentum'. In proposition 12, however, he says 'Weights ... have equal momenta or gravitate equally', and in two at least of the student copies, though not in Gregory's original, momentum is used in this sense in the following corollary¹⁵⁴. More seriously, in the introduction to this part 2, Gregory uses momentum in this sense three times without using the equivalent construction on 'gravitare'. In this part, he says, we will determine the proportion between the momentum of a weight lying on an inclined plane and its momentum when hanging freely; we will find the proportion necessary between weights lying on variously inclined planes, such that their momenta will be equal. The bodies in question are all stationary-momentum, as defined in part one by the product of mass and speed, is meaningless in this context. Since it was necessary to alter Dechaies' propositions to avoid this error, Gregory was well aware of the problem. The carelessness of allowing himself to commit it in the introduction not once, but three times, is inexcusable. One wonders what his

¹⁵³ Dechaies op cit (69) def. 1 p.489.

¹⁵⁴ EUL MSS Dc.6.18. La. 111. 170.

students, with no prior knowledge on this point, could make of it.

However, leaving aside this error in what is virtually the only non-derivative piece of writing in this section, we have a competent introduction to the dynamics of weights on planes, suited to the abilities of Gregory's students.

Gregory refers also in this section to Stevin's Statics, book 1 proposition 19. Dechaies mentioned this proposition in the equivalent place in his work, but he did not cite proposition 19 of book 1. The ability to pinpoint this proposition might suggest a familiarity on Gregory's part with this work. However, he does not seem to have used it anywhere else in these lectures.

2.8.4 Part 3 : Mechanica

This part deals with the five so-called simple machines: the lever, the winch, the pulley, the screw and the wedge. All five had been discussed in Wallis' De Motu and Gregory generally follows this treatment with some propositions from Dechaies. His treatment of the lever differs a little from these authors, however.

First, Gregory introduced a term for which Wallis had found no need - potentia. He defined it as

'Potentia is that quality by which anything tends to a different place from that in which it is, whether the tendency is upwards or downwards or sideways, or, at length, in any direction at all. Nor does it matter if this potential is innate to whatever it is in, or otherwise acquired or impressed on it by something else'.

We may say that potentia is anything, whether natural or artificial, which produces a tendency in something else to move. Gravity is thus

a kind of potentia and, says Gregory, since it is the most regular and uniform, we use it to measure any other potentia. Another definition identified directio potentiae, i.e. the direction in which it acts.

Gregory had previously used potentia in his definition of momentum ('potentia in corpore producendi motum'), in the same way as Wallis had used it in his definition of vis motrix. There we might translate potentia simply as potential, or perhaps power, but a different meaning has been given to it here. In connection with simple machines it is best translated as 'force'.

The origin of the term probably lay in Dechaes Cursus, where it is used in this same way, although undefined. In the analysis of simple machines it is equivalent to Wallis' use there of the term vis. Probably it was Gregory's confusion (or appreciation of Wallis' confusion) over vis which led him to use instead Dechaes potentia.

However, further confusion arises, when Gregory repeats Dechaes' phrase vires potentiae. For example, in his first proposition on the pulley, based on Dechaes proposition 5 (p.422), Gregory says

'Innumerable fixed pulleys neither increase or decrease the vires potentiae'.

We would say that a fixed pulley has a mechanical advantage of 1, or that the force applied to it must be equivalent to the resistance to be overcome. In this, and similar cases, we may best translate vires potentiae either as the ability of a force to overcome resistance, or simply as force. The use is consistent in Dechaes and in the propositions Gregory takes from him.

However, in the propositions taken from Wallis, Gregory has generally altered vis or vis motrix simply to potentia, used in the

same sense as vires potentiae in the propositions arising out of Dechalet's analysis. Clearly, the two uses are not altogether consistent. The definition of potentia could embrace either use, but not both. Since vis is undefined in Gregory's lectures, the expression vires potentiae can only be described as extremely vague.

Moreover, Gregory does not always change Wallis' vis to potentia. In his second proposition on the winch, for example, adapted from Wallis' proposition 2, p.972, he takes his statement of the problem straight from Wallis:

'To move a given weight with a winch, by a given
force [data vi]'.

Thus, although Gregory makes praiseworthy attempts to avoid the undefined term vis and replace it with potentia, he is not uniformly successful. In this chapter three, vis, potentia and vires potentiae are used interchangeably. As with the confusion which arose in part two over the use of momentum Gregory's problems arise principally from the attempt to impose a uniform terminology on propositions taken from different sources. The problem is compounded here by his wish to avoid vis.

In his treatment of the lever, Gregory differed from both Wallis and Dechalet. To them, a lever was a straight inflexible rod, resting on a fulcrum. To Gregory, it was two straight inflexible rods, with a fulcrum at their (immobile) join. These arms might, or might not, lie in a straight line. Moreover, Wallis derived his principles of the lever from his previous analysis of the balance. Gregory discussed the lever first and introduced the balance as a special case of the principle of the lever.

He began his analysis with a correct statement of the equilibrium conditions obtaining in a 'crooked' lever, easily derivable from Wallis' discussions of the balance. He drew on both Wallis and Dechaules but, because he had taken a more general form of the lever, his statements are more general. Also, although his propositions can be found (at least implicitly) in these sources, his use of them was much freer than in parts one and two.

He also refers in the scholium to proposition 1 on the lever to Archimedes De Aequiponderantibus, propositions 6 and 7, justifying the assumptions made there about the directions of forces. In proposition 7 he mentions the accurate balance made by Boyle, but this was also mentioned by Wallis from which source Gregory had undoubtedly taken the reference.

The discussion of the remaining four machines can be traced more directly to Wallis with some of Dechaules' work. Like them, he states clearly that a machine works because we are applying a smaller force, but over a greater distance, than we would need to do without the machine. (In other words, a machine does not increase the amount of work put into it, but employs it more effectively.) Machines can also be used to apply a force at a required point, or to harness a non-human force, such as that of a river or of a horse.

It is possible that Gregory used a third source for this section. His definition of a lever, and occasional propositions such as his reduction of a pulley to a lever are not found in his major sources. However, none of these points were especially new; he may have learnt them at Aberdeen, or from his father.

Apart from the confusion over vis and potentia, the principles of

simple machines are clearly presented here. Their uses are explained and the variety of their applications described. The popularity of this part of the course is implied by the number of copies we have of it; excepting those on practical geometry, these were probably the most read of Gregory's lectures.

2.8.5 Part 4 chapter 1 : De Gravium descensu libero

This chapter introduces a new source into Gregory's lectures; Huygens' Horologium Oscillatorium. Consisting of only five propositions, it gives Huygens' kinematics of free fall, itself largely based on Galileo's work. Also, some definitions and scholiums were taken from Wallis, such as the reiterated determination not to discuss the cause of gravity.

Proposition 4 describes the experiments of Galileo, Riccioli, Mersenne and Huygens on the rate of free fall, while proposition 5 gives further details on the experiments of Huygens. Riccioli's experiments were described by Dechaies¹⁵⁵, and Huygens' were in his Horologium. Gregory refers to the third Dialogue for Galileo's experiments, but he might easily have known of them through a secondary source. Similarly Mersenne's experiments, of which Gregory gives no details, were well-known. He will not repeat these experiments, however, since they involve the use of the pendulum, whose principles he has not yet discussed.

There were two final scholiums to this part. The first mentions air resistance and its effects on such experiments as these. A similar discussion is found in Dechaies. The second summarizes

¹⁵⁵ Dechaies op cit (69) prop. 21, p.483.

an article of Leibniz's against the Cartesians, and refutes it.

The particular point with which Leibniz took issue here was the Cartesian contention that ^{quantity of motion} ~~momentum~~ is always conserved. He showed that if two bodies of 1 lb. and 4 lbs. were dropped, 4 feet and 1 foot respectively, they would each possess the same quantity of vis motrix, but would have different momenta. To measure vis motrix, Leibniz used the axiom that a body acquires enough speed in falling to raise itself again to the height from which it fell. Thus vis motrix may be considered as the product of weight and height, mh for a body of weight m , falling from a height h . ^{Quantity of motion} ~~Momentum~~ is the usual mv where the body has acquired a speed v in falling. Clearly then, ^{quantity of motion} ~~momentum~~ and vis motrix cannot both be conserved. Since vis motrix is conserved, ^{quantity of motion} ~~momentum~~ is not. (Today, we would talk of the conservation of energy: here, the kinetic energy $\frac{1}{2}mv^2$, measured by the work done on the falling body, mh .)

But l'Abbe de Catalan, who had previously attacked Huygens' analysis of centres of oscillation, which Leibniz claimed was vindicated by his argument, objected to Leibniz's analysis. De Catalan said that this definition of vis motrix was acceptable only when the motions compared were carried out in equal times, as in simple machines. (Where the conservation of what we call work is the basic principle of analysis). However, it does not apply in cases such as this where the motions are carried out in unequal times, where the momenta are therefore necessarily unequal.

Gregory summarized Leibniz's argument, and answered it in a way very similar to Catalan's. His language is less argumentative than that of the two Continental scientists, and he quantifies Catalan's

arguments somewhat, claiming that we should divide the product of weight and height by time taken to find the true vis motrix. Nevertheless, his argument is essentially Catalan's.

However, Gregory refers Leibniz's article to the Acta for 1686, where it had gone unanswered¹⁵⁶. It was in the Nouvelles de la Republique des Lettres that Leibniz's article, translated into French, had appeared six months later with Catalan's reply¹⁵⁷. Since Gregory cites only this first publication, it might seem that he was unaware of Catalan's argument.

Among Gregory's papers, however, there is a manuscript containing the French version of Leibniz's article and most of Catalan's reply¹⁵⁸. It is undated, and is not in Gregory's hand, so he may have received it only after he had given these lectures. The Abbé's article is not complete in this paper, but enough is said to make the trend of his argument clear. Publication is not mentioned, so Gregory may have been unaware that both these articles had appeared in the Nouvelles.

Granted the general lack of originality which Gregory displays in these lectures, it seems unlikely that he would have taken the trouble to devise this answer to Leibniz. His researches at this time were taken up with his 'second method' of quadrature, and he would have had little time or incentive to follow up metaphysical arguments. Indeed, he was never to show any inclination for such

¹⁵⁶ AE (March 1686) 161-3.

¹⁵⁷ Nouvelles de la république des lettres (September 1686) 996-1003.

¹⁵⁸ CU8, RG fo. 104.

discussions, but generally avoided them wherever possible. Moreover, if we do not assume that Gregory received the paper containing these articles before he gave the lectures we must find a reason why he seized on this topic rather than any other. Many other discussions of this nature were going on, but nowhere in any of his lectures (not just those on mechanics) does he trouble to discuss them.

It seems most likely that Gregory received this paper from a friend on the continent, before he gave these lectures and without knowing that Catalan's article had been, or was to be, published. The topic was relevant to his lectures and did give his students some idea of the dissension which lay behind many of the concepts they were being taught. He simply extended Catalan's argument a little, and inserted the discussion into his lectures.

This would be merely another example of Gregory's use of sources which he did not always name. (Though, if he received Catalan's article as a private communication and believed it was unpublished, there would have been little reason to cite his sources). However, the passage was later to be published in his name.

In 1734, John Eames and John Martyn published a volume abridging the Transactions. As their first paper in mechanics they inserted this scholium wherein Gregory summarizes Leibniz's and Catalan's arguments¹⁵⁹. This paper, they note, is from Gregory's mechanics lectures, and was never previously published. We learn from Martyn's preface that Eames communicated this paper, described as 'the late

¹⁵⁹ John Eames and John Martyn (editors) The philosophical transactions, 1719-1733, abridged 6 (London 1734) 275-6.

learned Dr. Gregory's Discourse upon Motion'. The impression which the paper gives, both of the general standard of Gregory's mechanics lectures and of his ability to argue abstract points is, to say the least, misleading.

In fact, this is the only discussion of such a point in Gregory's lectures. Further, it was almost certainly taken from Catalan's paper. Gregory's repetition for his students of an argument from a source he did not trouble to acknowledge, led, almost 50 years later, to the publication of his purported 'Discourse upon Motion'.

Moreover, two further points arise out of this scholium. First, we have further evidence in the Martyn and Eames publication of an interest in Gregory's lecture notes. To find this passage, and to decide it was the most suitable for publication, John Eames must have made a fairly close study of at least part of Gregory's lecture notes.

Secondly, Gregory is arguing here, even if in Catalan's footsteps, on the Cartesian side. Certainly, having made wide use of the principle of the conservation of momentum in part one, he may have been afraid that allowing Leibniz's argument would have meant abandoning this principle altogether. Nevertheless, for whatever reasons, Gregory is clearly grouping himself with the Cartesians here, in spite of Leibniz's implication that to do so was to deny Huygens' analysis of the centre of oscillation - an analysis which Gregory was to discuss uncritically in part five of these lectures. Here, in the session 1686-87 when these lectures were given, we have further evidence of the Cartesian influences on Gregory's science when he was a young man.

2.8.6 Part 4 chapter 2 : De Motu Projectorum

For this chapter on the motion of projectiles, Gregory

introduced another new source, Torricelli's De Motu Gravium. First, though, the introductory suppositions were taken from Huygens' Horologium. These were followed by two standard propositions on the form of the parabola, which might have come from any writer on conics. Thereafter, this chapter follows Torricelli's discussion. Some alterations are made; perhaps it was Dechaies' example which persuaded Gregory to consider horizontal projection first, before the general case. Several similar deviations from Torricelli appear, but the theoretical treatment of projectiles which Gregory gives is all, at least implicitly, to be found in Torricelli. Indeed, the majority of the propositions are copied verbatim and most of the proofs are only minimally recast. The two propositions (28 and 29) on the use of the military square in directing a canon attribute the invention of this square and its improvement to Torricelli. The square is described in the final section of the Italian's De Motu Gravium, but Gregory does not mention the work itself.

Propositions 11 and 12 did not stem from Torricelli, however, and these give practical rules for directing a canon's fire and describe bombs. These propositions are not in any of Gregory's usual sources, but they are not technical and might be considered general knowledge.

In a final scholium, Gregory considers air resistance. The laws given in this chapter hold well for small flights, but are not so accurate over long distances. This must be attributed to air resistance. We have no reason to suppose that the air is anything but homogeneous, and as such, it is reasonable to assume that its retarding effect on a body is uniform. So a projectile should

probably be analysed in terms of a uniform acceleration downwards, and a uniform deceleration in the direction of projection. This analysis has been performed in a tract entitled Tentamina Geometrica de Motu Penduli et Projectorum, where the path of a projectile is again identified as a parabola, but of a very different form from that found in the discussion above.

The tract which David refers to was that of James Gregorie, his uncle. It had been appended to a tract published under the pseudonym Mathers¹⁶⁰. However, David does not mention the author of the tract, though it seems unlikely that he was unaware that this appendix, at least, was his uncle's work. He seems to have made no further use of it, though, not even in the final section which discusses the motion of a pendulum, which is the main topic of the appendix.

2.8.7 Part 5 : The pendulum

This section consists of three chapters. The first, like the entire section, is untitled and discusses again descent through inclined planes. Unlike part two, however, this discussion is kinematic, and it is extended to descent through a series of variously inclined planes, and thus to descent through a curve. Chapter two 'Pendula' applies this analysis to the pendulum and chapter three 'De horologiis automaticis pendulo instructis et mensura universalis' (On automatic clocks constructed with a pendulum and the universal measure) considers practical applications of the pendulum.

¹⁶⁰ Patrick Mathers The great and new art of weighing vanity (Glasgow 1672).

These lectures were first given in the session 1687-88, after Gregory had read the Principia. However, this work is certainly not an integral part of the lectures. Gregory had ended his introduction to part five by saying

'Finally, if time permits, we shall give the universal and absolute doctrine of all these things, outlined in Newton's Principles of Philosophy'.

In the event he did no more than touch on Newton's work, using the scholium after the laws of motion for its description of the use of a pendulum in experiments on impact, but without mentioning his source, and only briefly outlining the relevant points of Newton's investigations in his final scholium. There is no systematic account of Newtonian principles, nor is the major part of these lectures based on them.

Instead, most of the work comes from Dechaless Cursus. Book 3 of his Staticae follows the same general pattern as these lectures and supplied most of the propositions. Some also came from Huygens' Horologium, especially his description of the mensura universalis, his method of finding the 'true' length of a pendulum and his discovery of the isochronous property of the cycloid. All these discoveries are attributed to Huygens. Moreover, an additional theorem has been inserted after proposition 7 in Gregory's original copy, and it refers to a result of Huygens on descent in a cycloid, citing proposition 25 part 3 of the Horologium. One proposition (22 of chapter 1) states that a 3, 4, 5 triangle is the only right angled triangle whose other two sides are run through in the same time as the hypotenuse. This is Torricelli's proposition 44. In chapter 2, propositions 4 and 5

have come from Wallis, propositions 32 - 34 (pages 614 - 615)', and discuss the forces required to raise a pendulum or move it beyond the perpendicular. These appear somewhat incongruous and certainly irrelevant in the otherwise kinematic treatment.

Only Huygens and Newton are cited, and that only when Gregory wishes to produce results from these sources which he will not prove in the lectures. Dechaies, Torricelli and Wallis are not mentioned.

It is noticeable that here, far more than in the earlier parts, Gregory simply adopts unaltered the presentation of his sources. His proofs are closer to those of the originals and almost every proposition is a verbatim copy. The impression here is that Gregory was considerably less sure of his subject matter than in the earlier parts.

The final two scholiums are particularly interesting, for they deal with the refinements Huygens and Newton have made in the doctrines set out already. Gregory had discussed Huygens' invention of a universal measure, which was to be one third the length of a pendulum which completed an oscillation in one second. As Huygens then believed gravity to be a worldwide constant, this seemed to define a length which would be the same all over the world. Gregory had also introduced the problem of allowing for the effect of the bob's weight and shape in calculating the period of a pendulum. He followed Huygens' concept of a centre of oscillation and quoted his results for spherical bobs, pointing out that Huygens had also considered any plane or solid bob. Thus we derive Huygens' idea of a moveable bob by which the centre of oscillation of a pendulum may be altered, and its period thus regulated. Now, in the first of these final scholiums,

Gregory points out that oscillations in circular arcs are not 'really' isochronous. He describes 'Clariss' Huygens' device of cycloidal restraining cheeks, which constrain the bob to oscillate in a cycloid and so to perform truly isochronous oscillations. This device, Gregory points out, will be of extreme usefulness in astronomy, geography and navigation.

In the second scholium, Gregory turns to Newton's Principia. (He had previously used the scholium after the laws of motion, but without referring it to Newton). Now Gregory points out that the above work has been built on two assumptions. First, we have assumed that gravity is a constant force at any distance from the earth's centre, and that that centre is infinitely distant. (That is, we have assumed a constant downwards acceleration on any body, wherever situated, and that this acceleration always acts in a direction parallel to itself). Neither of these are strictly true, and Newton does not assume them. Thus he has deduced that the restraining cheeks on an isochronous pendulum should not be shaped as the cycloid formed by rolling a wheel along a straight line, but as that formed by rolling a wheel about the circumference of another wheel. Moreover, he has shown that the strength of the gravitational force varies in places at different distances from the equator, and so Huygens 'universal measure' will not work. (Here, Gregory attributes the device to Huygens, although he had not done so when he first described it). This follows from the spheroidal shape of the earth. However, Gregory concludes, any further investigation of these matters would require a deeper study than there is now time for.

These few comments are the full extent of Newton's influence on

Gregory's mechanics lectures; a cautionary postscript to the investigations of Huygens. As he says, the study of Newtonian mechanics would necessitate a much deeper investigation - in particular, it would necessitate a thorough knowledge of the most advanced mathematical techniques. His students did not have that knowledge, and Gregory wisely refrained from doing more than hinting at the improvements Newton had made in mechanics. In just the same way he would hint at the Newtonian advances in astronomy when he gave the final part of his astronomy lectures in two years time, but would give no details of these advances. Gregory had nothing but praise for Newton's work, but he gave no detailed account of that work. Wallis and Dechaes, with Torricelli and Huygens were suitable authors for Gregory's students; Isaac Newton was not.

2.9 Lectures in practical geometry and surveying: Geometria Practica

Gregory's lectures on practical geometry were certainly the most popular of his courses. First given in 1685, they were published 60 years later in translation, with additional notes. McLaurin is named as editor of this work, but Irving suggests that at least some of the notes were those of Robert Stewart, professor of natural philosophy at Edinburgh University¹⁶¹.

As testimonial we need only quote the advertisement appended to the published copy.

'This Treatise was composed in Latin about sixty years ago by Dr. David Gregory, then Professor of Mathematics in the University of Edinburgh, where it has been constantly taught since that time, immediately after Euclid's Elements and the plain Trigonometry, as proper for exercising the students in the Application of Geometry to Practice'¹⁶².

The translation had been made by 'an ingenious Gentleman when a student here'¹⁶³. The work was extremely successful, and by 1796 had run into eleven editions.

The place which these lectures had in the syllabus of 1745 was that which Gregory had always designed for them. He had placed them there in his papers on education and opened the lectures themselves by

¹⁶¹ Irving op cit(11) ii 264.

¹⁶² David Gregory A treatise of practical geometry (Edinburgh 1745) Advertisement.

¹⁶³ Ibid.

mentioning that work on Euclid's books 1 - 6, 11 and 12 and trigonometry were now completed.

For his students Gregory had particularly recommended Ozonam, or else Dechales or Clavius¹⁶⁴. Any or all of these works may have been used as sources for these lectures for all cover broadly similar ground.

The lectures, as their title implies, are pre-eminently practical and part one especially is geared to the needs of the surveyor. This part, on the measure of lines and angles, has an additional treatise on surveying, and is largely concerned with the use of instruments. Examples are given of the use of a geometric square, a plane mirror, two staves, a geometric quadrant or a graphometer in measuring heights and distances, and several other methods are also indicated. The emphasis throughout is on practice and the merits of ease of calculation as against accuracy of instruments are discussed in general terms. When introducing his section on surveying, Gregory says

'a surveyor will improve himself more by one day's practice, than by a great deal of reading'¹⁶⁵.

This attitude is present throughout the lectures.

Parts two and three discuss the measurement of area and volume, but are mainly restricted to the elementary rules applicable to simple solids. The method of indivisibles is referred to as a possible way of proving Archimedes' rule for the volume of a segment of a spheroid,

¹⁶⁴ Ozonam Cursus mathematicus 5 vols. (London 1712) vol. III seems to be an English translation of the work to which Gregory refers.

Dechales op cit (69) i bk V.

Clavius Geometria Practica (Moguntiae 1606).

¹⁶⁵ Gregory op cit(162) 56.

but no details are given of the method¹⁶⁶.

Circle measurement is discussed; in order to find either the circumference or the area from the diameter. In the former case Archimedes' ratio of 7 to 22 is mentioned. However, a far more exact measurement is that which Ludolphus van Ceulen gives¹⁶⁷. This approximation to π is mentioned in all three of Gregory's recommended texts, but only Ozonan gives as many decimal places as Gregory does.

Further, Gregory gives the series $1 - 1/3 + 1/5 - 1/7 \dots$ for the ratio to one which a circle has to the square of its diameter; that is, for the value of $\pi/4$ ¹⁶⁸. (As the editorial notes point out, this series is not in fact very practical, as it converges only slowly). This was the series which Gregory had derived in his Exercitatio Geometrica, published in 1684, and had attributed to Leibniz, ignoring his uncle's prior claim; a point which the continental scientists were quick to seize on when the Newton-Leibniz calculus priority dispute blew up. (see Chapter 3). In these lectures, though, Gregory makes no attempt to show a method of discovery, but simply presents the series to his students.

These lectures, then, are not new and original. They explain clearly and competently the practice of surveying and its related skills. The relevant information, normally found in bulky tomes, is gathered in a slim volume. Gregory does not seem to be indebted to any one source and this is probably largely his own compilation.

¹⁶⁶ Ibid 130.

¹⁶⁷ Ibid prop. 22, 54-6.

¹⁶⁸ Ibid prop. 7, 97.

He was, after all, the son of a land-owner who took a keen interest in mathematics. Although the evidence we have suggests that Gregory's practical attainments were not impressive, it seems highly likely that his father would have made sure he could cope with the rudiments of surveying. He was therefore probably able to call on his own experiences when writing these lectures.

Indeed, we find Gregory here at his best. He gives a lucid account of a topic of which he is thoroughly master, and directs his students towards a clear practical end. These lectures would cause no revolution in philosophy, but they would teach students to survey, and their popularity was well-deserved.

2.10 Lectures on Trigonometry and Logarithms

As a basis in mathematics after arithmetic and Euclid, Gregory recommends trigonometry and logarithms, followed by practical geometry. Spherical trigonometry might be included here, or be left until it was necessary for the study of astronomy.

We generally find these courses together, and also with the practical geometry lectures. In all, we have 7 copies of the plane trigonometry each in a notebook also containing the practical geometry. 5 of these have the lectures on logarithms and one also has the lectures on spherical trigonometry, which is the only copy we have of these lectures. All were probably first given in 1686.

Gregory had recommended a preliminary study of Theodosius Elementa Spherica, which follows Euclid's Elements and precedes trigonometry in the collections of both Dechales and Clavius. Either of these works, or Briggs' Trigonometry would do for trigonometry, while logarithms should be studied in Napier's own works. The writings of Henry Briggs and Adrian Vlacq were also useful for logarithms.

In fact the content of Gregory's lectures might be found in any of these sources. The trigonometry consists of basic definitions and rules for the solution of right and oblique angled triangles. The account of logarithms and their use is confined to a brief account of the underlying principle and rules for the use of tables of logarithms. The method of calculating logarithms given in Mercator's Logarithmotechnia is recommended as a saving in labour, but it is not part of Gregory's plan to describe here how these calculations are performed. Those who wish more information may consult the authors of tables of

logarithms.

Thus, the lectures are a clear and concise introduction to the solution of plane and spherical triangles and to the use of logarithms, but the emphasis is heavily on practice rather than theory. Trigonometry and logarithms were to be useful tools for Gregory's students, to whom use is paramount. There is no point in the introduction of ideas such as the logarithmic curve, or even the connection between logarithms and the hyperbola. Trigonometric relationships are not developed beyond the point where they are of immediate use in the solution of triangles.

As Dr. Lawrence points out, it has been suggested several times that John Keill make use of these tracts by appending them to his 1715 edition of Euclid¹⁶⁹. However, the Biographia Britannica tells us that Keill regarded these tracts as his best work¹⁷⁰. In fact, the two sets of tracts bear little resemblance to each other. Keill's treatment is far more extended and theoretical than Gregory's. He gives Newton's series for sine and cosine and discusses the origin of logarithms. Naturally, the straightforward rules which Gregory gives may be found in Keill, but there is no particular similarity of presentation or language.

¹⁶⁹ Lawrence op cit(34) 29. See, for example, John Gregory A father's legacy to his daughters (Edinburgh 1788) 20.

Euclid's Elementorum libri priores sex, item undecimus et duodecimus ex versione Latina Frederici Commandini quibus accedunt trigonometriae planae et sphaericae elementa. Item tractatus de natura et arithmetica logarithmorum (Oxoniae 1715).

¹⁷⁰ Biographia Britannica (London 1757) 4 2806 note L.

Irving refers to an earlier edition of 1700, to which these tracts were appended, but the only edition before 1715 which I have traced is that of 1701, referred to by Dickinson, whose imprimatur is dated 1700¹⁷¹. This edition does not contain the tracts on logarithms and trigonometry. If the edition mentioned by Irving existed, it seems to dispel all doubt of Keill's having used Gregory's tracts. He could hardly have done so without acknowledgement in Gregory's lifetime.

Pitcairne had made a similar charge against Keill's Introductio ad Veram Physicam (Oxford 1702). Writing to Walkinshaw in 1709 he said that

'Keil stole his Principia Physicae vera word by word from Dr. Gregorie's dictats'¹⁷².

However, unless a substantial volume of Gregory's lectures are now lost, this is simply untrue. The letter continues to criticise the work itself and to suggest that, since Whiston's theological arguments had been given to him by Newton, Keill's attack on them had been an attack on Newton, and it was this which had lost him the Savilian Chair which he might otherwise have had on Gregory's death. Pitcairne then turns to the anatomy lately published by James Keill, where his main charge against it is again plagiarism. As John's Introductio

¹⁷¹ Irving op cit(11) ii 276.

Dickinson op cit(13) lviii.

Euclid's Elementorum ... ex versione Latina Frederici Commandini (Oxoniae 1701) (Imprimatur Oxon 1.11.1700).

¹⁷² Pitcairne to Walkinshaw 27.12.1709.

BM MS Sloane 3216 fos. 174, 5.

had then been in print for seven years, we would be immediately suspicious of Pitcairne's motives in making this charge at this late date. In the context of the letter it is clear that he is simply casting up as much scandal as he can about the two Keill brothers, and it is hard to take his charges against the Introductio seriously. Moreover, Gregory was still alive when this work appeared, yet remained on terms of complete amiability with John Keill. He would have been the first to recognise that the work bore no similarity to his Edinburgh lectures, and only such to his Oxford lectures as was unavoidable for two ardent Newtonian scientists discussing similar topics.

In sum, Gregory's lectures on trigonometry and logarithms were concise and clear rules of operation. They bear little relation to the extended treatment given by Keill in the tracts appended to his Euclid. Perhaps the guess that Keill had used Gregory's tracts arose out of the coincidence of subjects. Perhaps it had more personal motives, as did Pitcairne's charges that the Intruductio came from Gregory's notes. This latter charge was also unfounded.

2.11 Hydrostatica

These lectures are the most problematical of all those assigned to David Gregory. As they are the most explicitly Newtonian of all the courses, it is especially important that their author be determined. Unfortunately we simply cannot be certain that these lectures were the work of David, or of his brother James (though they must have been the work of one of these two). Although these lectures set out Newtonian principles of hydrostatics, however, and make some attempt to apply them to the situations examined, the work of Wallis is still a more important part of the work. Certainly these lectures indicate a higher degree of interest in and enthusiasm for Newton's work than do any of the others, but it would be difficult to argue that they constitute an introduction to the principles of Newtonian philosophy.

We have only three copies of these lectures, two of which ascribe them to David's brother James. The other suggests they are David's. Francis Pringle's notebook contains an undated copy of 'Hydrostatica a D.D.J. Gregorio dictata'. Here, the second 'D' and the 'J' have been added later, so that the heading originally read 'a D. Gregorio dictata'. This implies that Pringle first believed the lectures were David's, then discovered he had been mistaken and altered the heading accordingly. I have not discovered the significance of the title 'D.D.' before James Gregorie's name, instead of the simple 'D.' for Dominus which we would expect, but he is often so named. Pringle was otherwise accurate, where he named an author, in attributing David's lectures to him, and James' theses to him.

Two notebooks in Aberdeen also contain the lectures. One is dated 1740 and also contains lectures by David Gregory and Robert Stewart.

The first set of notes in 'Geometria Practica a Domino Davide Gregory in Academia Edinensi Matheseos Professore Scripta An. Dom. MDCCXL'. Then follow the hydrostatics lectures, entitled 'Hydrostatica a Domino Jacobo Gregory in Academia Edinburgensi Matheseos Professore Dictata. Transcripta An. Dom. MDCCXL'. Finally the notebook contains the hydrostatics lectures of Robert Stewart, Edinburgh's professor of natural philosophy, also written in 1740.

The third notebook is generally titled 'Systema mathematicum authore D. Gregory' and has no date. It includes David Gregory's lectures on trigonometry, logarithms, practical geometry, astronomy along with those on hydrostatics and gnomonics. As remarked above, since this is the only copy of the gnomonics lectures, we must, if we believe the hydrostatics lectures were James' believe the gnomonics might also be. However, there is nothing to suggest that James could not have written them, and no difficulty in this supposition.

On the evidence of these notebooks alone, the lectures appear to have been James'. The notes which give him as author are far more detailed than the notebook which throws them in as part of a course of David's. Both the notebooks which mention James also mention David and so the students who copied them were aware of the need to distinguish between the two brothers. We have no evidence of such awareness in the book which attributes them to David.

Some further evidence on the side of James' authorship might appear from the content of the lectures. These are more Newtonian in approach than any of the lectures we know David gave, while James had given enthusiastically Newtonian graduation theses in 1690. However, David's private teaching was probably Newtonian, and it was

lack of opportunity as much as unwillingness which prevented him from including Newtonian science in his lectures. Secondly, these hydrostatics lectures acknowledge their sources in a much more liberal manner than the other lectures. Both Newton and Wallis are frequently referred to in them, while we have seen that David was generally most reluctant to name his prime sources.

On the other hand, we have no lectures which we know were James' with which we can compare these. We do know, though, that lectures were often attributed to him which had first been given by his brother. It is possible that he used David's lectures in his teaching when he replaced him in the Edinburgh Chair, so that in the course of time they were attributed to him. This may have happened with the hydrostatics lectures.

Moreover, we know that David was involved in teaching at least some of the content of this course. The fourth libel laid against him in 1690 reads

'4^o He is ane habituall swearer for instance in trying lately some experiments at the air pumps, he putts in a pigeon, and when by pumping out the air the bird beguine to fent, he cryes See ye not? God, she is dieing¹⁷³.

Gregory's answers to this charge deny the blasphemy, but not the experiment¹⁷⁴. Just such experiments with animals in the exhausted receptacle of a vacuum pump are described in these hydrostatics

¹⁷³ B24.

¹⁷⁴ B25.

lectures.

Moreover, by June 1688, one at least of David Gregory's students was familiar with the experiments of Torricelli on air pressure which are also described in these lectures. Charles Sinclair's graduation speech of that year described such experiments as performed by Torricelli, Huygens and Hooke. This speech covers ground not discussed in the lectures, and we can see from the 1690 speeches made on a similar occasion that such speeches were not in general drawn from lecture courses. However, the speech suggests that Gregory found the Torricellian experiments suitable for at least some of his students to study. Sinclair's speech makes no mention of Newtonian principles of hydrostatics, however. (These speeches are discussed in 2.12).

In sum, then, the lectures might first have been given by David or by James. The evidence seems to be weighted somewhat in favour of the latter, but it is not conclusive in either case.

The section Hydrostatica from Wallis' De Motu and section 5, book 2 of Newton's Principia supply the material for these lectures. Of their eleven propositions, the first two are taken from Newton and the remainder from Wallis.

Newton had defined a fluid as any body whose parts yield to any impressed force and in yielding move amongst themselves. His first proposition then stated that all the parts of a homogeneous and unmoved fluid contained in an unmoved vessel and compressed on every side will (ignoring condensation, gravity and any centripetal forces) be equally pressed on all sides, and will remain in position without any motion arising from that pressure. Proposition two considers a spherical fluid, homogeneous at equal distances from its

centre lying on a concentric spherical base with each of its parts gravitating towards the centre. The base will then sustain a weight equal to that of a cylinder of the fluid whose base equals the surface of the inner sphere and whose height is the depth of the fluid. Many corollaries follow from this, including the behaviour of bodies immersed in fluids of a different specific gravity¹⁷⁵.

This definition and these propositions are copied verbatim as the introduction to the hydrostatics lectures, and their source in Newton's Principia is acknowledged. There are only two alterations; the concepts of absolute and relative gravity are given in the lectures as preliminary definitions, whereas Newton introduces them in the sixth corollary to the second proposition. Also corollaries seven and eight to this proposition, which deal with centripetal forces other than gravity, are omitted from the lectures.

Wallis' introductory proposition is less general in concept. It establishes that the surface of a heavy fluid under equable pressure will assume a spherical surface, concentric to the earth's and, if disturbed, it will return to this form. Any parts of the whole which are subjected to greater pressure than others will move from that pressure. Anything which is true of the upper surface is true of the layers parallel to it. Thence he derives the behaviour of bodies in fluids of a different specific gravity to their own¹⁷⁶.

Newton's analysis, which concentrates attention on the parts of a fluid, puts the behaviour of bodies in liquids of different

¹⁷⁵ Newton op cit(89) Def. 8 props. 19, 20, 290-6.

¹⁷⁶ Wallis op cit (132) Chap. 4 'De Hydrostaticis' prop 1. 1032-3.

specific gravity on a firmer footing than Wallis'. Also, Newton's discussion is much more quantified. However, both are designed to introduce particular aspects of the behaviour of fluids and are suited to their individual aims.

Newton goes on to apply his principles to the study of our atmosphere, and to deduce a possible relationship between altitude and density. Then he considers the relationship between density and compression in fluids composed of mutually repelling particles. The remainder of the book considers resisted motion, and fluid dynamics in the propagation of waves and the behaviour of vortices.

Wallis, however, intended his introduction to enable him to explain the Torricellian experiments and similar ones on air pressure and vacuums and the use of hydraulic machinery. His basic premises might not have been suitable for the studies Newton was to make, but they were perfectly adequate to deal with the phenomena he wished to discuss.

The hydrostatics lectures, after giving Newton's introduction, turn instead to Wallis. His introductory proposition becomes proposition 3 of the lectures and they continue with the air pressure experiments, vacuum pumps and hydraulic machines which Wallis described.

After the second Newtonian proposition the author of these lectures said

'Although it may be clear that much of the following flows like so many corollaries from these two propositions (which we have transferred from the Principles of the most famous Newton), yet it is pleasant to add the demonstrations of these too, lest

anything is omitted which might contribute to their certainty and evidence'.

Of course, the following propositions could be derived from Newtonian principles, but this is certainly not the easy and evident business which this author implies. Nor does he use these principles in his proofs, but follows Wallis' demonstrations. The use of Wallis is not altogether verbatim as the copy of Newton had been, but there is only one significant difference; the lectures use the previously defined term 'specific gravity' throughout. Newton used this term but Wallis did not.

On only two occasions is any use made of the Newtonian principles set down at the beginning. Firstly, whatever the shape of the inverted vessel in which it is contained, air pressure will sustain a 29" column of mercury. Wallis explained this by pointing to the column rising above the vessel's base and claiming that the sides of the vessel help to sustain any additional mercury. The author of the hydrostatics lectures attempted to employ the analysis in terms of innumerable horizontal layers which Newton had used in his second introductory proposition. However, this application is crude and unconvincing¹⁷⁷.

On the second occasion, the lecturer discusses the reasons why we do not feel the weight of the air¹⁷⁸. The 9th corollary to Newton's second introductory proposition had explained this in terms of relative weight and this had been inserted in the lectures. The

¹⁷⁷ 'Hydrostatica' scholium to prop. 7.

¹⁷⁸ Ibid scholium to prop. 9.

author refers back to it, but he says, the fact that we are unaware of this weight may be so astonishing to some people that it is worth discussing further. Whereupon, he gives Wallis' answers to the question.

In the lectures, Newton's Principia is mentioned once, Wallis' Hydrostatica twice and on one occasion Wallis is referred to without the work being mentioned. The experiments of Torricelli, Boyle and Mersenne are all mentioned, but these references were probably taken from Wallis.

How much can we say that these lectures introduce Newtonian philosophy? Of course, the more famous aspects of the philosophy; universal gravitation, the composition of light, the three laws of motion and so on, are not mentioned. Nevertheless, these principles are part of Newton's original analysis of the physical world, and as such, although a small part, are nevertheless part, of Newtonian philosophy.

However, the emphasis is not on these principles, but on the experimental facts of air pressure and similar phenomena. These lectures are chiefly designed to introduce new, exciting work; the existence of a vacuum, the weight of the air. They are designed to explain how hydraulic machines work. Only as a secondary purpose are they designed to introduce Newtonian principles.

Nevertheless, the very irrelevance of this secondary purpose to the primary one is important. The primary purpose of these lecture could have been achieved just as well without introducing Newton's work, which was therefore introduced for its own sake only. That is, these lectures constitute a deliberate attempt to introduce a part, at least,

of Newton's work.

It is hard to say what impact these lectures would have had, and this again depends largely on their author. If David gave them, it must have been in or before 1691, when he left Edinburgh, in which case they would, for most of the students hearing them, have been a first introduction to Newton. If James gave them it would have been as Edinburgh professor of mathematics, in or after 1692. It was about this time that discussion of Newton's philosophy was beginning to enter the regents' lecture courses, and his work would probably no longer have been entirely new to James' students.

In any case, the important point is that these lectures are certainly now wholly Newtonian. His principles are introduced, but not developed further. The author may have found Wallis' principles easier for himself to handle; certainly they were easier for his students. Also, the material to which Newton applied his principles was of far less immediate importance and interest for under-graduates than Wallis' accounts of experiments and machines.

If these were David's lectures, then they were by far the most Newtonian of his courses. Yet, even here, Newtonian science is unimportant compared with giving his students practical attainments and teaching them to understand everyday phenomena.

2.12 Graduation speeches

In seventeenth century Edinburgh, graduation was a lengthy process. The students had spent the previous four years under one regent, and he was not involved in their final examination. Instead the other three and the Regent of Humanity examined the boys in certain subjects and arranged them in ranks analogous to our classes of Honours degrees. At first these ranks were announced publicly, but this led to complaints from those whose sons had not done very well that their boys had been publicly humiliated! Accordingly, by mid-century these results were announced before only the Town Council, Ministers and Masters.

Laureation was generally on a Monday in late June or early July. The day was spent in public disputation of themes set by the boys' regent in his graduation theses. In theory the learned audience produced arguments against which the class defended the theses, but in practice the boys were probably prepared to take sides against each other in case this audience participation failed. By the late 1680's the boys were also giving individual graduation speeches.

The graduation list had already been drawn up, and nothing said or done on this day could alter it. However, a good display in this exercise (which was, of course, entirely conducted in Latin) might win a young man valuable patronage in his chosen career, for many learned and wealthy men would form part of the audience¹⁷⁹.

Gregory, then, as professor of mathematics, was not formally involved in this display. He did not examine students, nor prepare

¹⁷⁹ A. Grant The story of the University of Edinburgh ... 2 vols. (London 1884) i 151-5.

topics for them to debate. However, among his papers there are four speeches given at graduation in 1688 and 1690. These, especially two of the three given in 1690, go considerably beyond Gregory's public lectures and give us an insight into his private teaching.

We do not know why Gregory copied these particular papers in his own hand and kept them among his papers. Perhaps these were the work of particularly promising pupils or perhaps, with a free range of choice, no others had previously chosen scientific topics. The sudden popularity of mathematics among students graduating in 1690 can be easily explained on a non-scientific basis. Gregory had in that year become something of a cause célèbre among the students for his disagreements with the Town Council.

Whatever Gregory's reason for keeping them we have here four examples of work done by his students on relatively advanced scientific topics; in particular on the work of Huygens and Newton. The influence to tackle these subjects must have come from Gregory; no-one else at the University at that time had the knowledge to direct the boys to these studies.

The first speech was given by Charles Sinclair on his graduation in 1688¹⁸⁰. He devoted his talk to the barometer, explaining its basic principles clearly and discussing practical problems and improvements on the basic design. The work of Torricelli, Huygens and Hooke is mentioned.

Then we have three speeches given on June 30th 1690. It was this laureation of which Gregory said

¹⁸⁰ Cl88.

'June 30 Massie was infinitely petted at Mr. Kennedies oration and theses in the forenoon, satt in the desk in spite of Kennedie wher he disoblidged the provost, and was ridiculed by the students',¹⁸¹.

Kennedy was the regent of the graduating class, and Massie another regent who had been trying to prevent those boys who would not promise to omit all praise of David Gregory from their speeches from speaking at graduation. Clearly there were deep political undercurrents below the theses that day!

The speeches were given by John Falconer, Laurence Oliphant and William Cooper¹⁸². The first may have been related to the Falconer who secured Lord Tarbat's interest for Gregory over the visitation. The second was perhaps a future relation by marriage, perhaps even a future brother-in-law. For the third, as of Charles Sinclair, we cannot even guess at identities. All we know certainly is that each of these students graduated at the time they gave these speeches¹⁸³.

Falconer's chosen subject was cosmology, and his themes pro-Copernican and anti-Cartesian. The first men had assumed that the world is geocentric and this developed into the Ptolemaic system. However, he contends, Pythagoras and Heraclides 'developing philosophy by geometrical reasoning and not placing too much faith in their senses' numbered the earth as a planet and made the sun immobile. Aristarchos not only introduced the heliocentric system but also based it on a

¹⁸¹ B23.

¹⁸² C189, C190, C193.

¹⁸³ Laing op cit(20) 139.

theory of vortices. Thus everything Copernicus said - except for a few calculations - is found in Aristarchos, while Descartes' entire physical system is no more than that of Aristarchos! Since vortices are now used to explain so many phenomena - such as gravity, electricity and cohesion - it is worth while to enquire into their foundations and to decide whether they are compatible with celestial phenomena. Here two standard Newtonian arguments against vortices are introduced. They contradict Kepler's second law generally and, in particular, they imply a faster motion at aphelion than perihelion. Finally he quotes from the Principia (citing Newton but not naming the book) the biting comments on the role of vortices - not to explain, but to confound, the heavenly motions¹⁸⁴.

Oliphant's theme was the quadrature of Hippocrates' lunula - an elegant problem of squaring a curvilinear shape. However, it used only standard Euclidean techniques and so did not in itself go beyond Gregory's standard lecture course. But Oliphant continues by pointing out the usefulness of such an exercise. Isaac Newton and Christian Huygens are now using the quadrature of curvilinear areas to solve many problems in natural philosophy, such as those involving attractions, or centripetal and centrifugal forces. Only the ignorant or the philophraster could now exclude geometry from physics, or contend that physical quantities could not be represented by mathematical lines.

The argument was not new - the mathematization of physics had been a long process and few indeed would by that time have denied its usefulness. However, the paper shows at least an awareness of the

184 Newton op cit(89)bk.2 scholium to prop. 53 400.

mathematical difficulties in the work of Huygens and Newton, and of their concepts of attraction, centripetal and centrifugal force, none of which were discussed in Gregory's lectures.

Cooper's speech discusses light as an example of the value of experiment in science. The Peripatetics, performing no experiments, could only discuss whether light were substance or accident. Only when geometers took hold of the subject did the sciences of optics, catoptrics and dioptrics arise, where properties of light were derived from experience. Descartes is treated more kindly than in Falconer's speech. His theory of light as a pressure provided a good explanation for the non-interference of light rays travelling in opposite directions. His assumption that the speed of light is infinite was, he believed, founded on sound observational evidence. But Huygens and Newton have now shown that light travels in waves and that its speed is finite. Römer's measurement of this speed is carefully described, but the Dane is not mentioned, leaving the strong implication that this was the experiment of either Newton or Huygens.

Thus, although Falconer's historical arguments are a little confused at times and Cooper is apparently unaware of who actually performed the observation of Jupiter's satellites by which the finite speed of light was detected, these three speeches show at least some acquaintance with the most modern authors. Indeed, Huygens' theories of light are in his Traité de la Lumière which had only appeared that year, so Cooper's sources are especially recent.

All four of these speeches go beyond the limits of Gregory's lectures, especially Falconer's cosmology and Cooper's optics. They

not only mention the work of Newton and Huygens, but discuss the value of mathematics to science and the essential nature of experiment. These boys were probably only eighteen years old, and quite possibly less. Yet they were able to discuss the most modern developments of science. When accused before the visitation committee of superficial teaching, Gregory had answered 'the proficiency of scholars is the best proof of pains'¹⁸⁵. If these four students were typical he might have been sure of his proof!

There are certain similarities between these speeches and the graduation theses James Gregory gave in 1690¹⁸⁶. The use of Newton's Principia and Huygens' Traité de la Lumière is one. Also, both the theses and Falconer's speech give similar historical introductions and both quote Newton on vortices. This might provide further evidence for David's influence on his brother James, especially with regard to his adoption of Newtonianism.

There was no-one but Gregory at Edinburgh in 1688 and 1690 who would have introduced the boys to these subjects. Newtonian science was not part of David's lecture course, but these speeches imply that he nevertheless discussed it in his private lessons.

¹⁸⁵ B25.

¹⁸⁶ James Gregorie Theses Philosophicae (Edinburgh 1690).

2.13 The Oxford Lectures

The Savilian statutes required that Gregory lectured at least twice a week for three quarters of an hour, and based his lectures on Ptolemy's Almagest. A copy of these lectures was to be deposited with the University. However, the lectures which we have for this period by no means correspond to such a syllabus¹⁸⁷.

For the five years between November 1692 and November 1697 we have only 26 lectures, barely enough for the first session. Moreover, one of these lectures, dated 8th July 1693 does not really belong to the set. It was given on Commencement Day, at the graduation ceremony, and discusses the approaches to science seen in the work of those dealing with its different branches. Another was first given at graduation a year later on 7th July 1694 and takes as its theme the usefulness of geometry to science. However, it also includes a discussion of observations of the fixed stars and was put into the appropriate place in the sequence of public lectures and given again on 12th November 1696. Neither of these two lectures mention the Almagest, although it is frequently referred to in the others.

Including the second of these two, we have 25 lectures; 6 given in the session 1692-93, 7 in 1693-94, 3 in 1694-95, 4 in 1695-96, 4 in 1696-97 and 1 in November 1697 after which Gregory remarked that all future lectures would be taken from his Astronomiae. This work could not conceivably be seen as a commentary on the Almagest, and its use contrasts strangely with Gregory's determination in these lectures to adhere to the design of Ptolemy's work.

¹⁸⁷ A.U.L. MS 2206 8. These lectures are described in P.D. Lawrence and A.G. Molland 'David Gregory's inaugural lecture at Oxford' Notes and records of the Royal Society 25 (1970)143-178.

Except for the graduation lectures, all were given between October and May, generally between November and February. None were given later than December 8th nor earlier than January 19th, nor were any given in March. Of course, there are too few lectures for us to deduce from this a short academic year with long Christmas and Easter vacations, but the lectures would certainly fit such a pattern.

After five years these lectures had reached chapter nine of the Almagest. They follow the order in which Ptolemy discussed various topics, but they introduce frequent diversions into physics. The first two lectures are introductory, and then the following three discuss Ptolemy's chapter two and the effects of refraction on the appearance of the stars. The following eight lectures discuss Ptolemy's chapters three to five on the shape, size and position of the earth, introducing much contemporary work. The next nine lectures cover the period from December 1693 to May 1696 - probably as long as any under-graduate would spend with the astronomy professor. Their topic is purportedly Ptolemy's sixth chapter on the stationary earth and the arguments from terrestrial gravity which he uses to support this. In argument, Gregory details many contemporary alternative explanations of terrestrial gravity culminating with that proposed by Fatio de Duillier. The final five lectures are on a mixed set of topics. The first, on air pressure, follows from these discussions of gravity. The next two discuss the fixed stars, and the first of these was the lecture initially given on Commencement Day 1694. The last two discuss chapters seven and nine of the Almagest. These consider the first motion, the Tychonic and Egyptian systems and the division of the circle into 360 degrees.

Besides Copernicus and Geber, the modern authors recommended by the statutes, Gregory drew on many other sources. He used Gassendi, Kepler and Vitellio on refraction, many ancient authors on the shape of the earth with Galileo and Torricelli on the same topic, Newton on centrifugal force, Stevin on equilibrium, Pythagoras and Aristarchos on the moving earth, and the observations of Tycho, Huygens, Riccioli, Lansberg and Kepler. For his discussion of terrestrial gravity, Gregory referred to Epicurus and Lucretius, Gassendi, Descartes, Huygens, Newton, Kepler, Varignon, Perrault, Cusa, Longomontanus, Borelli and Fatio. Clearly the range of sources employed was far wider than in the Edinburgh lectures.

There were several other differences between these lectures and the Edinburgh ones. Clearly, the adherence to Ptolemy's Almagest enforced a different pattern on these lectures. The methodical presentation of topics which marked the Edinburgh lectures is almost totally absent here where subjects seem to be introduced in almost random order. This constraint also leads to the diversions into topics which do not properly belong to astronomy lectures. Gregory, from his apologies, seemed also to feel the inappropriateness of some of his discussions, but did not cut them short. The statutes enjoined that he should also lecture on topics such as optics, geography, gnomonics and so on. In the lectures we possess there are diversions into both optical and geographical topics and it seems that there can have been no separate lecture courses on such subjects.

Most important though, is the difference in emphasis, following the difference in aim, between these two sets of lectures. The practical element, so important in the Edinburgh lectures, is missing here,

and replaced by a thorough investigation of the basic assumptions on which the Edinburgh astronomy lectures were based. In the introduction to the Oxford lectures Gregory had announced his intention to explain the use of equant devices in calculating the planetary motions, but after five years of lectures he seemed to be no nearer to such a discussion. Instead, he had spent these lectures examining the arguments for and against the heliocentric universe. Of course, most of the diversions were largely irrelevant to this theme, but, from a strictly astronomical point of view, these lectures established the heliocentric position, with only the addition of some observational description such as that of the stars.

The Edinburgh astronomy lectures, on the other hand, began with a heliocentric system. The world was stated in the first line to be filled with Cartesian vortices, each centred on a sun. From this starting point the motions of the planets were swiftly sketched in to form the basis of the calculations explained in parts 3 and 4. The contrast could not be clearer; the Edinburgh lectures were for practical minded boys who wished rules and calculations, while the Oxford lectures were for the philosophically minded who wished to form some conception of the physical universe and to weigh in their own minds the reasons for accepting the views they did. These Oxford lectures are virtually useless for the would-be practical astronomer, but they are altogether more thoughtful and thought-provoking than the Edinburgh lectures.

This different approach shows also in the use Gregory makes of his sources. At Edinburgh, these were used without acknowledgement, even when quoted verbatim. Where Gregory disagreed with their treatment,

as in Wallis' use of momentum he simply altered it without argument to his own view. Yet such disagreement was rare, as he normally avoided anything in the least controversial. At Oxford the sources are all referred to, and discussed in full before being accepted or rejected.

An interesting exception to this general rule, however, is Newton. While these lectures are not overtly Newtonian and mention his work only rarely, the underlying assumption is that of a Newtonian universe. This assumption is not discussed, probably because it was simply beyond doubts and questions. The discussion of gravity shows this most clearly.

The reasons for the differences between these sets of lectures can only lie in their audiences. At Edinburgh, Gregory was lecturing to a regular group of under-graduates who looked to him to provide the practical rules he gave them. It appears that his Oxford audiences must have been somewhat different.

There is some evidence of an Oxford and Cambridge tradition of professorial lectures which were simply too difficult for under-graduates¹⁸⁸. The resulting sparsity of audience may have led the professors to lecture only rarely. Non-University teachers such as William Oughtred and John Caswell would supply the needs of those students who wished to study mathematics and science. There were also a number among the regent masters who taught science to their pupils so that not all education in the 'new philosophy' took place outside

¹⁸⁸ Phyllis Allen 'Science in English Universities of the 17th century' Journal of the history of ideas X (1949) 219-53 p.242.

the universities¹⁸⁹. Yet the professors' public lectures were not expected to supply this practical education.

When Gregory wrote his 'colleges' scheme in 1700, he felt it necessary to emphasise at once that he proposed his scheme

'Without discouraging any other person in the University, that teaches or intends to teach Mathematics'¹⁹⁰.

This did not mean John Wallis, Savilian professor of geometry, who was most enthusiastic about the scheme, but the many masters attached in some degree to the Oxford colleges who provided the students with a mathematical education.

This very paper shows that Gregory had not lost sight of his utilitarian goals in education when he moved to Oxford. The reading list for his students and his advice to Balliol's masters were also written when he was at Oxford. All these papers proposed well-ordered, broad schemes of education in mathematics with the emphasis on practical attainments. But they refer to the students' private study, to the teaching of the masters of Balliol or to the astronomy professor's private teaching; they do not refer to his public lectures.

There were many masters and tutors at Oxford who provided the sort of basic instruction which Gregory had given himself at Edinburgh when all the mathematics education was his responsibility. As Savilian professor his lectures were not concerned with such mundane considerations. They were designed to introduce controversial issues,

¹⁸⁹ Ibid 232.

¹⁹⁰ Charlett to Pepys 5.10.1700 Tanner op cit(56) 90.4.

and modern work, and to discuss these for the benefit, perhaps, not of under-graduates but of the other masters. Their aim was not the handling of practical astronomical calculations but a deeper insight into cosmological (or optical, geographical or physical) speculations.

As examples of these lectures, I have looked at the sections on refraction and gravity. The first is typical of these lectures and illustrates the points I have mentioned above. The other is also interesting for other reasons. It is almost the only occasion on which Gregory speculated on such a topic without apparently consulting Newton and merely echoing his views. However, the Newtonian influence is interesting here, largely because it is not discussed. Gregory argues strongly and in detail over the heliocentric hypothesis or the possibility of a mechanical explanation of gravity, but he simply takes for granted that any such explanation must account for the Newtonian hypothesis of universal gravitation.

2.13.1 Refraction : Oxford lectures 3 - 5

Refraction was discussed in the Edinburgh lectures on both optics and astronomy. In the former it was fully examined, on the basis of the Cartesian physical explanation. The astronomy lectures merely introduced it as a caution. Because of atmospheric refraction, celestial phenomena appear higher in the sky than they really are, but the reader was referred to the optics lectures for an account of the general phenomenon.

In the Oxford lectures this result of refraction, the important one to practical astronomers, is barely mentioned. Instead, Gregory takes as his starting point Ptolemy's attempt to explain why objects seem larger near the horizon, which seems at first to contradict the

supposition that the heavens are spherical. It is this phenomenon around which his discussion centres. At Edinburgh he had simply mentioned the apparent displacement of objects in the heavens and given the generally accepted explanation; at Oxford he examines and discusses in detail the various hypotheses put forward to explain this phenomenon.

Ptolemy's explanation had been based on vapours between us and the horizon which have a similar effect on the star to that of water on a stick immersed in it. Gregory argues against the vapours on such grounds as that they would also increase the apparent angle of separation of celestial phenomena. Thus the horizon would seem to be more than 360° . The analogy with a stick in water is false, too. Gregory shows that under these circumstances, a body would appear smaller near the horizon. He does not deduce this from first principles, but bases it on the conclusions found in the writings of optical authors. By his arguments on refraction, Gregory says

'Ptolemy although a most outstanding astronomer, yet distinguished himself as a mediocre enough physicist'.

What refraction does do, he explains, is make objects seem higher than they are. (Again he gives no detail beyond saying that it follows from the refraction of light rays towards the perpendicular on entering the atmosphere). Since this effect is greatest nearest the horizon, it makes objects there appear oval and somewhat smaller. Further, since the light must pass through more atmosphere to reach the observer from the horizon than from the zenith, the brightness of an object will be diminished and this will also make it seem smaller.

However, although Gassendi, by measuring the sun's shadow, has

shown that it does in fact seem smaller near the horizon, to the naked eye celestial bodies seem larger in that position¹⁹¹. Modern instruments may have shown that the appearance to the naked eye recorded by Ptolemy is an illusion, but it must still be explained.

The explanation that, because of the earth's shape, bodies really are further from us at the horizon than at the zenith might hold for the moon. However, such an effect would be too small for the explanation to cover the sun as well. There is also the effect of the atmosphere in absorbing light rays and so making bodies seem smaller near the horizon, and this would have a greater influence in the case of the sun than of the moon. This was the explanation Gassendi favoured.

However, we must still explain why, to the naked eye, bodies seem larger near the horizon. Vitellio came nearest to the correct answer, and Gregory wonders that Gassendi, with the work of Kepler and Vitellio to draw on, should have missed it. Instead, the 'learned and acute' Gassendi, in a tract expressly written on this topic, could only relate the difference in size to a larger or smaller aperture in the pupil of the eye¹⁹². Near the horizon, less rays reach the observer and so the pupil is dilated, causing the luminary to seem larger. But those versed in optics know that a dilation of the pupil will make an object seem more vivid by letting more rays through, but it will not not make it seem larger.

Gregory's explanation of this appearance is based on the way in

¹⁹¹ Pierre Gassendi De apparente magnitudine solis humilis et sublimio epistolae quattuor (Paris 1642).

¹⁹² Ibid.

which animals judge distance. They proceed trigonometrically by calculating from the angle at which an object is seen by each eye. From its distance and the angle it subtends its size is calculated, but all this is done so swiftly that we are unaware of the process. Distance can in theory be judged by one eye by the adjustments necessary for distinct vision, but this is a very inaccurate procedure. The mind can only judge distances accurately when an object is close enough for the distance between the eyes to be sensible with respect to the observer's distance from that object. Clearly this is not so for celestial bodies.

However, distances are also judged by comparison with objects lying between the observer and the object whose distance is being judged. For this reason, mountains always seem nearer when there is nothing lying between us and them by which we can judge their distance. An analogy might also be drawn with our perception of time, but this is not the place for such speculation. In this way, the luminaries appear closer near the zenith, where there are no intervening objects, than they do near the horizon. Since they subtend the same angle at the eye in each case we judge that they are larger near the horizon.

A similar effect follows the loss of light rays as discussed earlier. Luminaries seem less bright, and therefore more distant at the horizon. Again, since they subtend the same angle at the eye, this makes them seem larger to us.

Thus, Gregory's treatment of refraction displays the characteristics noted above. It is largely devoted to questions irrelevant to the practising astronomer and only briefly mentions the effect of refraction in making heavenly bodies appear higher than they are, which is the

important point to an observer. He introduces the most modern work on the topic, Gassendi's, and discusses his views along with these of Ptolemy and, briefly, of Kepler and Vitellio. In the end he forms his own opinions on the most probable explanation and presents them to the audience. Finally, Newtonian astronomy is irrelevant to the discussion, and Newton is not mentioned.

2.13.2 Terrestrial gravity; lectures 13 - 21

Gregory discussed gravity in 9 of his Oxford lectures, given from 7th December 1693 until 14th May 1696. Thus, they account for over a third of his full Oxford course of astronomy lectures. The first two were delivered before his visit to Newton in May 1694, the remainder afterwards, but this appears to have had little influence on the lectures. These, although only once mentioning Newton's name, are based on an unquestioning Newtonian picture of gravity.

In the mechanics lectures which he gave at Edinburgh, Gregory had said

'Gravity is a motive force downwards, or towards the earth's centre. What the principle of gravity may be as a physical consideration, we shall not here enquire. It is enough that we understand by the word gravity that which we detect with the senses as the force which moves a heavy body downwards'¹⁹³.

This had, of course, been quoted directly from John Wallis' De Motu¹⁹⁴, but the sentiment was also Gregory's; speculation on the physical cause of gravity was not a suitable topic for under-graduate lectures,

¹⁹³ Gregory 'De Motu' def. 10.

¹⁹⁴ Wallis op cit (132) chap.1 def. 12.

where it was enough simply to know the phenomenon existed. Just so, he had declined to discuss the nature of light in his lectures on optics.

However, this simple assumption was not sufficient for the Oxford lectures, where the Savilian Professor was evidently expected to take a more searching look at the foundations of his science. The sixth book of Ptolemy's Almagest had given several arguments to support the view of the earth as maintaining a stationary position at the centre of the world, most of which Gregory had answered in a previous lecture. However, Ptolemy had also argued the point from an Aristotelian principle of gravity. That is, the phenomena of weight and levity arise from the attempt of bodies to seek their own place. The proper place of earthy matter is the centre and so it tends to move towards it, thus forming our spherical earth, about a stationary central point in absolute Aristotelian space. To counter-act this argument, Gregory needed to advance only one alternative view of gravity which did not depend on the position of the earth. For the business of his argument is not to disprove this Aristotelian view, but only to point out that there are alternatives. As such, his discussion of every alternative adds nothing to his professed theme.

His real theme, though, is rather different - the impossibility of devising a mechanical hypothesis of gravity. Not only the hypotheses he examines but all such are to be rejected. Instead, gravity will be shown as an innate quality of matter, instilled there by the Creator. Now, the relevance of his wide survey of such explanations is apparent. His general arguments against mechanical hypotheses are now bolstered by particular examples and the arguments against them.

A contradiction arises only when he comes to Fatio's explanation, which Gregory presents as the only possible one out of these hypotheses, although he is still inclined to accept none of them. The decision to include this hypothesis was probably made at the last minute and we shall see below what may have prompted him to make it.

Thus the lectures are presented as a commentary on Ptolemy, but are really concerned with rejecting all mechanical explanations of gravity. Nevertheless each begins with a *résumé* of the argument so far and its connection with the Almagest. Often the lectures end with a similar comment, but in between Ptolemy is forgotten. Gregory is simply taking pains to assure his audience that he is in fact following the Savilian statutes.

The first lecture shows how the problem arises out of Ptolemy's discussions and the second tells us the sort of gravity which Gregory will discuss. Gravity 'as used by Kepler and later by Newton' is

'a mutual corporeal action between cognate bodies to union or conjunction, and so in this opinion any body tends to any other, since there is no specific difference between them'.

As such, he adds, it differs little from the atomist concept of Epicurus, as it was developed by Lucretius and Gassendi.

This was said by Gregory on 8th February 1693/94, but a year earlier Newton had written to Richard Bentley

'Tis unconceivable that inanimate brute matter should (without ye mediation of something else wch is not material) operate upon and affect other matter without

mutual contact⁴; as it must if gravitation in the sense of Epicurus be essential and inherent in it. And this is one reason why I desired you would not ascribe innate gravity to me'¹⁹⁵.

Yet Gregory also ascribed just such innate gravity to Newton, and coupled it with the name of Epicurus.

The Principia invoked no etherial mechanisms to produce gravity. Instead such phrases as

'since the Earth, the Sun and the Planets gravitate mutually towards each other ... ',

'the forces of the sun to disturb the motion of the moon'

or 'the attraction ... of the Moon towards the Earth in the syzygies is the excess of its gravity towards the Earth over the solar force'¹⁹⁶,

all seem to suggest an innate force of attraction in celestial bodies whereby they act directly on each other. It was natural that Gregory, like Bentley, should assume that such an innate attraction was Newton's view.

The following lectures, which were given after Gregory's reconciliation with Newton in May 1694 (see Chapter 4) reaffirm several times Gregory's view that gravity is an innate quality, but Newton is

¹⁹⁵ Newton to Bentley 25.2.1693 NC III 406 263-6.

¹⁹⁶ Newton op cit (89) bk.3 prop.12 p.418, prop.25 p.434, prop.28 p.439 in my translation.

not once mentioned. We do not know whether they discussed the topic. Certainly it is not mentioned in the notes Gregory took of that meeting in May, although he does say that the philosophy of Epicurus and Lucretius is 'true and old but was falsely interpreted by the ancients as atheism'¹⁹⁷. A similar comment was added to the Notae, which specifically referred to Epicurus' views on gravity¹⁹⁸. Newton's view of himself as rediscovering truths known to the Ancients is reflected in Gregory's memoranda of the 1690's, but the references are to the manifestations of universal gravity, rather than its mechanism.

It may be, then, that Gregory never realized that Newton did not share his view on the nature of gravity, but continued to suppose he was arguing Newton's point of view. Alternatively, the two may have discussed the topic, and yet Gregory decided nevertheless to retain his viewpoint. Of course, Newton was by no means committed to a mechanical hypothesis, and must have been well aware of the problems in devising an acceptable one. He may even have encouraged Gregory to continue arguing the case against such an hypothesis in his lectures. He would certainly, in that case, have asked Gregory not to name him. His complaint to Bentley did not arise from a dislike of the principle of innate gravity itself, so much as from a reluctance to be publicly linked with the principle. Such a situation would explain why Newton's name appears only in the second lecture of this series, although Newtonian principles are frequently referred to.

¹⁹⁷ 5-7.5.1696 c44 RG fos 68, 9 NCIII 446 334-6 p.336.

¹⁹⁸ Notae 122.

In the remaining lectures, Gregory examines alternative hypotheses always returning to the same one; gravity is an innate quality of matter, instilled there by the Creator. First the views of Epicurus and Lucretius with Gassendi's extension by analogy with magnetism are found inadequate to express all qualities of gravity. Next he looks at Descartes' explanation and the extensions of it presented by Perrault Varignon and Huygens. It is only after these have been examined that he turns to the general refutation of such theories and finally to Fatio's hypothesis.

Descartes' explanation was contained in part 4 of his Principia¹⁹⁹. The earth, he said, was surrounded by swirling celestial matter whose speed of rotation (and consequently centrifugal force) was greater than that of the earth. Thus terrestrial matter was displaced downwards towards the centre of the earth as celestial matter rose to take its place. Perrault's hypothesis introduced a somewhat different celestial matter, to which circular motion was necessary and which therefore did not undergo centrifugal force. Its varying speeds of rotation produced the effect of gravity when terrestrial matter escaped from faster to slower moving areas. By making these speeds vary in planes perpendicular to as well as parallel to the earth's equator, this scheme directed gravity towards the earth's centre and not merely its axis as Descartes' had done²⁰⁰. Huygens and Varignon also avoided this problem.

¹⁹⁹ For accounts of theories of gravitation based on vortices see E.J. Aiton The vortex theory of planetary motions (Belfast 1972) London

²⁰⁰ Claude Perrault Essais de physique (Paris 1680)

Huygens' hypothesis was first propounded in 1669, along with an earlier version of Perrault's, but did not appear fully in print until 1690 when it was appended to the Traité de la Lumière. He retained the notion of centrifugal force, and set his fluid rotating in every possible direction. It was constrained to this motion by its spherical container. Huygens' mathematical analysis of the resultant forces, backed up by experimental evidence produced a mathematical framework for Descartes' qualitative description.

Varignon's theory appeared first in 1688 in the Histoire des Ouvrages des Scavans and two years later was published on its own²⁰¹. He found the cause of gravity in the particles of the air whose rapid motion is the cause of the earth's fluidity. A body is assaulted on all sides by these particles, and thus remains in equilibrium in the horizontal plane. However, when a body is near the earth, the much larger quantity of air above it than below it means a greater pressure above arising from these collisions and the body is thus pushed downwards. Clearly, this is a much less satisfactory explanation than Perrault's and Huygens'. Leibniz and Huygens criticized it in their correspondence, where Leibniz said it was equivalent to supposing that the force of a river of given speed was as its length²⁰².

²⁰¹ Histoire des ouvrages des scavans (July 1688) 351.

Pierre Varignon Nouveau conjecture sur la pesanteur (Paris 1690)

²⁰² Huygens to Leibniz 21.4.1691 and Leibniz to Huygens 27.5.1691
 Huygens Oeuvres complètes de Christiaan Huygens publiées par la
 Société Hollandaise des Sciences 22 vols (La Haye 1888 - 1950)
 X 87 & 99.

Gregory dismisses all these hypotheses, pointing out particular objections to each as well as general objections. Descartes' system (like many other such) implies a constant wind about the equator. Perrault's implies that the moon completes its period more swiftly than the earth and so a month is shorter than a day. The objections to Varignon's hypothesis are clear, and Gregory could quote the experiments of Mersenne and Petit against it²⁰³. Especially, since it depends on the prior existence of a large material body, this hypothesis could not explain the formation of the universe out of primal chaos.

Huygens' theory is another matter. In 1693, not long before he began this series of lectures, Gregory had visited the Dutchman, and his respect for his work was immense. He was the man whose Horologium gave us the 'symptoms, passions and properties of gravity and weight'. The ideas of such a man on gravity cannot be ignored. Gregory devotes two lectures, in fact, to the opinions of Huygens on the faults in other systems and to the details of his own, based on a concept of centrifugal force and relevant experimental evidence. This hypothesis, formed 'ab optimo quidem Geometra' is much to be preferred to all previous ones. However, it, too, has its faults.

The effort to recede from the centre which causes gravity would mean (for Huygens supposes neither that the heavens are completely full with this mixture, or that its particles cannot be brought together) that the centre would soon become void of this matter, which would be piled around the sides of the container. Nor

203 René Descartes Epistolae (Amstelodami 1668) bk.1 no. 73.

is Gregory satisfied with the way this container constrains the particles to circular paths, for he does not believe, as Huygens does, that in this case it is easier for them so to move.

However, these individual criticisms are less important than these which apply to all mechanical explanations. Gregory detects seven properties of gravity which must be (and generally are not) explained by any hypothesis.

First, gravity is proportional to quantity of matter and all bodies are heavy. Thus we cannot (with Descartes, Huygens, etc.) accept a weightless celestial matter. This objection to Huygens' hypothesis had been raised by James Gregorie, David's brother, in his graduation theses of 1690.

Secondly, all bodies, as is well-known, fall at the same speed. Weights tend to other weights, not to points in space. Gravity must not contradict an 'inviolable law of nature', that actions have equal reactions. Thus attraction must be mutual. Fifthly, if we consider bodies as made up of impenetrable corpuscles, the action of any external force (such as these celestial matters) must be on the surface of these corpuscles. Thus their effects will be proportional to the surfaces of the corpuscles, or, assuming the same number of corpuscles in two bodies, to the surfaces of the bodies. This contradicts the known proportionality of gravity to quantity of matter, and so gravity cannot depend on any force external to the body itself. Next, as we see from the planets, gravity acts in a vacuum. Finally, gravity obeys the inverse square law, which applies also to other natural phenomena such as light. By analogy, gravity, too, is propagated rectilinearly in all directions from a centre.

Other general problems had been pointed out in previous discussions. For example, the force of gravity inside the earth's crust is directly as the distance from the centre, and this must be explained in terms of the earth as a body and not simply of its centre as a point in space.

Clearly the concept of gravity outlined here is Newtonian. Some of the properties, such as the changed proportion below the earth's crust, are not truly observed properties, but are corollaries of universal gravitation between all particles obeying the inverse square law. The law of action and reaction is itself Newton's, and might in any case be observed between a body and the celestial matter, without involving mutual attraction between bodies. This mutual attraction, like the inverse square law and the vacuum of free space are tenets of Newtonian philosophy, backed up by observational evidence, but none are directly observed and for the non-Newtonian none are the indisputable laws of nature which Gregory presents them as.

Moreover, his first demand is for an explanation in terms of a material substance which is itself subject to gravity. As he pointed out at the start of this discussion, however, the quest for such a substance will lead us through an infinite number of them, each producing gravity in the one before. To a philosopher such as Huygens there was no difficulty in simply supposing this substance not subject to gravity. Even to a Newtonian such as Fatio this seemed an allowable supposition.

From these arguments Gregory concludes that no mechanical explanation of gravity is possible. Gravity is an innate, internal

force, put into all matter by the Creator.

This lecture, given in April 1696, mentions for the first time the hypothesis of Fatio de Duillier. The remarks on it appended briefly to this lecture were subsequently deleted and the topic made the subject of the final lecture in the series given in May 1696. After all his earlier remarks on the subject, and the intimation that Huygens, if anyone, would be the man able to explain gravity mechanically, the sudden introduction of Fatio's hypothesis seems remarkably inconsistent. It is still not a perfect answer, but this explanation is the only mechanical one which one might accept. Yet, until April 1696, the hypothesis was not mentioned. A possible explanation for its sudden introduction appears from a history of this hypothesis²⁰⁴.

Fatio had followed his election to the Royal Society in May 1688, at the age of 24, with an account given two months later of Huygens' theory of gravitation. However, he had also (since late 1687 or early 1688 by his own account) been working on his own hypothesis, and he read this to the Society on 26th February 1690, in Edmund Halley's presence. He sent copies of it also to Newton and Huygens and in 1694 to Leibniz.

Huygens was not very impressed. He wrote to both de l'Hôpital and Leibniz that the hypothesis was like that of Varignon, and suffered under the same difficulties²⁰⁵. Nevertheless, when Fatio sent his

²⁰⁴ This hypothesis and the stages in which it was composed are discussed in Bernard Gagnebin 'De la cause de la pesanteur. Memoire de Nicolas Fatio de Duillier, presenta a la Royal Society le 26 fevrier 1690' Notes and records of the Royal Society 6 (1949) 105-60.

²⁰⁵ Huygens to de l'Hôpital 29.12.1692.
Huygens to Leibniz 29.5.1694 Huygens op cit(202) 354, 613.

theory to Leibniz he informed him that this was the hypothesis of which Huygens was then persuaded²⁰⁶. We might therefore pay little attention to Fatio's assertion that Newton believed

'That there is but one possible Mechanical cause of Gravity, to wit that which I had found out; tho' he would often seem to incline to think that Gravity had its Foundation only in the arbitrary Will of God'²⁰⁷.

Yet among Newton's papers there is just such praise of the hypothesis as the unique satisfactory mechanical explanation. This paper is dated by the Halls to the 1690's²⁰⁸.

Thus, the hypothesis was made public from early 1690, and was apparently viewed favourably by Newton as at least the best mechanical hypothesis.

David Gregory first learnt of it when he was in London in December 1691 waiting to take up the Savilian Chair. On the 27th of that month he took notes on Fatio's method of explaining gravity. These notes are not extensive and raise more questions than they answer, but they are an uncritical attempt to record the essentials of the method²⁰⁹. The next day his memoranda note that in the preface to a new edition of the Principia

206 Leibniz to Huygens 22.6.1694 Ibid 644.

207 Gagnebin op cit(204) 117.

208 King's College, Cambridge MS Add. 4005 fos. 28, 9.
See A.R. and M.B. Hall Unpublished scientific papers of Isaac Newton (Cambridge 1962) 313.

209 C86. Partly in NCIII 70 nl.

'Mr. Fatio ... will explain gravity acting as Mr. Newton shows it doth, from the rectilinear motion of particles the aggregate all which is but a given quantity of matter Dispersed in a given space. He says that he hath satisfied Mr. Newton, Mr. Huygens and Mr. Halley in it'.

However, he added later below this

'Mr. Newton and Mr. Halley laugh at Mr. Fatio's manner of explaining gravity'²¹⁰.

Gregory did not lose interest in the topic, but determined to ask Huygens' opinion when he visited Holland in 1693²¹¹. This opinion cannot have been favourable. Some time in or after April 1694 Gregory again noted that Halley thought nothing of Fatio's way of explaining gravity²¹². It is only in 1698 that we find any comments Gregory had directly from Newton on the matter, but then Newton told him that only Fatio's explanation derived the inverse square law²¹³.

It seems from this highly improbable that Gregory should suddenly decide in 1696 to discuss this method of Fatio's. As far as he is aware, Halley, and probably Newton, too, laugh at it. Huygens almost certainly had no very favourable comments to make on it. Moreover, Gregory's knowledge of this explanation was contained in notes made

210 RG fos 70, 71 NC III 381 191.

211 A8.

212 C55, RG fo. 79.

213 C62.

five years ago, which are confused and incomplete.

However, among Fatio's papers there was at one time a manuscript entitled 'Mon Manuscrit de la Cause de la Pesanteur quarto daté Oxford 1696'. Part of this manuscript contained 'une idée singulière de la Matière suggérée par Mr. Newton'²¹⁴. Fatio had in 1696 been for some years tutor to Lord Russell's son and perhaps his presence in Oxford was connected with this charge. In any case, he was in Oxford, writing a second draft of his hypothesis, in the same year that Gregory lectured on it. The two events cannot have been simply coincidence.

No doubt Fatio presented his new scheme to Gregory, in an improved form, so far as we can judge from his notes, to that Gregory had seen in 1691. Coupled with his own arguments Fatio could now report Newton's partial acceptance of the hypothesis. Thus Gregory was led to countenance it, and lectured on the hypothesis in its half-developed state of 1696.

In June 1690, Fatio had written to his brother that his hypothesis 'établit toute une autre idée de la Philosophie que celle que l'on a eue jusques à present'²¹⁵. This was a typical overstatement, but the theory certainly had some novel elements in it.

In the form in which Gregory gives it, Fatio's explanation supposes first that all bodies are of an extreme rarity, so that the gravitational medium can permeate them. Secondly, the ultimate particles of matter must be of such a form that their surfaces are proportional to their quantity of matter. (For so we avoid

²¹⁴ Gagnebin op cit(204) 119.

²¹⁵ Ibid 110.

objection 5 above). This is true for cylinders of a constant radius, if we exclude their bases from the measure of their surface. Thus we can posit infinitely thin cylinders for our ultimate particles for then both surface and quantity of matter are as their length. To account for the diversity of matter we may then combine the cylinders into a corresponding variety of forms. Since these forms will not break down into cylinders again we may call them 'atoms', the basic units of any particular material. Thirdly, all space contains minute particles which move swiftly in all directions. These particles are not reflected when they hit a solid body but adhere to it. (In other versions they lost most, but not all of their momentum.) They are so very small that in doing so they make no sensible difference to the size of the body.

Thus, any two bodies are impelled together since they screen each other from the particles. The greater pressure arising from the particles hitting their unscreened sides pushes the bodies together. Thus Fatio shows that gravity increases with the number of cylinders and so with matter. The pressure acts in straight lines from a centre and so obeys the inverse square law. Where this law operates between particles, the force of gravity inside a large mass is as the distance from the centre, Galileo's laws of free fall apply and generally the consequences of such a law derived by Newton hold true. If we suppose that the gravitational medium operates in a vacuum we have answered all of Gregory's seven objections. (Indeed, since Fatio's hypothesis is first mentioned in the lecture where these are set down, it is possible that they too were part of Fatio's paper, or that Gregory selected them with Fatio's answer in mind). It is

even possible that the decreased period of the moon (a small effect over a very long time) is the result of the accretion of these particles.

Yet says Gregory, although this hypothesis best satisfies the phenomena, there are other hypothesis which are less laughed at. This is only because those others are less geometrical and less exact. By this time, Gregory must have been aware that Newton was no longer among those who laughed!

There are, however, some objections. First of all, the device of the infinitely small cylinders is only an approximation. Since they are not actually geometric lines, their thickness must be accounted for and their surfaces would not be just as their quantity of matter. Secondly, Gregory produces his constant objection to such hypotheses. The gravitational particles are not themselves heavy. Other objections, he says, could be raised.

Thus, yet again, Gregory is back in his original position; mechanical hypotheses are impossible and gravity is an innate quantity of matter instilled by the Creator.

The account of Fatio's hypothesis given here differs in some details from later ones. However, its essentials - the rapidly moving particles permeating all matter and stopped, or almost so, on collision with corporeal bodies - remained unchanged. Gregory made no further use of it, and certainly made no attempt to develop it or to devise an alternative. Even in these lectures it was no more than the least objectionable hypothesis.

Thus we can find in these lectures a quiet Newtonianism contrasted perhaps to the strident Newtonianism of the graduation theses which David's brother, James, gave in 1690. The discussion

of gravity mentions Newton only once and the Principia not at all. It makes no attempt to argue the case in favour of the Newtonian theories of gravitation, accepting these as axiomatic well-known principles. This acceptance no doubt reflects Gregory's assumptions rather than those of his audience, but this calm assurance must have had some influence on those who heard him.

The attitude Gregory took to the central problem of explaining gravity was probably strongly influenced by Newton. As we have seen, his own view of gravity was that on which the Principia appeared to have been based. Yet the ultimate partial acceptance of Fatio's hypothesis, for which the previous lectures have in no way prepared us, was probably due to Newton's recommendation of this explanation.

The lectures as a whole are not especially Newtonian. They are concerned with producing modern ideas, some of which originate with Newton or Newtonian scientists such as Fatio. Sometimes, as in the discussion of refraction, Newtonian ideas are not relevant to his theme (though Newton's tables of refraction and comments on atmospheric density could have been forced into the discussion). Instead, he discusses Gassendi's views and does not mention Newton.

These Oxford lectures are very different from the Edinburgh ones. They were written with a different aim in mind and from a far wider range of sources which are differently used. They were expected to appeal to a more sophisticated audience who wanted metaphysical arguments instead of rules for calculating positions in the sky. Where relevant, they use Newtonian physics as an assumed basis to controversial topics. Newtonian physics itself is not controversial and so is not argued about.

Nevertheless, they are not attempting, anymore than the Edinburgh lectures did, to persuade anyone of the Newtonian philosophy. Compared, say, to Keill's Introductio ad Veram Physicam, which was based on his Oxford lectures, the lectures Gregory gave at Oxford were unimportant in the introduction of Newtonianism.

Ed. No.	Optics	Mechanics	Astronomy	Logarithms	Trigonometry	Practical Geometry	Hydrostatics	Miscellaneous	Remarks
La III 170		1-3: n.a. Fin: A.D. 1692							'Geo. Foulis' signature p.1.
La III 175	1684 DG								signed 'David Laine Edin. 1816'
De 7. 69			1&2; 3 ch.1 n.d. n.a.		Plane only n.d. n.a.	1705 n.a.			titled 'Notae Mathematicae'
De 6. 12	1694 DG	1-4; 1694 n.a.	1-3 n.a. 1693 pt.4 St. Andrews 1705 writ- ten by George Wood	DG n.d.	Plane only n.d. DG	n.d. n.a.	JG n.d.	De ratione studii DG 1695 Oxon. JG Theses Philosophicae 1690	Francis Fringles notebook
La III 570			1-4 1690 DG		Plane only DG 1690	DG 1690			titled 'Cursus Mathematicus'
De 6. 19		pt. 3 JG n.d.							
De 6. 18		pts. 1,2 Fin. Mar 1690 pt.3 Dec 1689 pt.4 Mar 1690							
De 5.57 De 1.75:	1683 (B11)	1&2 1684 (B12) 4 1686 (B15) 5 Dec '87- Jun '88 (B16)	1&2 (B7) 1685			n.a. n.d. (B6) 1685			DG's own copies of Edinburgh lectures
QA33 G8D1	Dec '83- Jun '84 DG	1&2 Dec '84-Jun '85 3 Dec '85- Jun '86 DG						Edinburgh inaugural lecture 10.12.1683 DG	titled 'lectiones mathematicae' 1683-1686.
QC361 G8	1684 DG								signed by James Browne
QA33 G8D2				n.d. DG	Fin. June 1727 n.a. Plane only	June 1727 DG			
QA35 G8L4		1&2 n.d. n.a. sum- marized	pt.4 n.d. n.a.						notes of David's nephew, David Gregory
QA503 G8				1696 JG at St. Andrews	1696 JG at St. Andrews Plane only	1696 JG at St. Andrews		English notes on Arithmetic and Algebra	titled JG's Geometria Practica 1696
2171			pts.1&2 pt.3 ch.1 DG n.d.	DG n.d.	DG. n.d. Plane only	DG n.d.	D.G. n.d.	Horolographia DG n.d.	DG's 'Systema Mathematicum'
672						DG n.d. but 1760 watermark + English addenda			Exlibris 1813 Gul: Henderson Presented '92 by Professor Jack
2206 6		1&2 1685 DG at Edinburgh							signed 'W. Wallace'
2206 9		3 Fin. Feb 1713 n.a.							
2206 7						DG Edin. 1740 + English notes	JG Edin- burgh 1740	(Robert Stewart's hydrostatics)	
Savile 97	Dec '83- Jun '84 DG at Edin.								signed F.P. Rigaud
Savile 98				1686 DG at Edin- burgh	Plane & Spherical 1686 DG at Edin.	1686 DG at Edin.			Copied c.a. 1710 Rigaud June 6th 1835.
131		1-5 n.d. n.a. sum- marized							
133		1-3 1686 DG at Edin. corrected by DG							Came to Christ- Church through David Gregory's son, David.
133a		4 1689 in DG's hand							

DG = David Gregory

JG = James Gregory, David's brother)

n.a. = no author cited

These initials are used to show to

whom authorship of the notes is

attributed.

Chapter 2

Appendix 2

Sessions in which Gregory's lectures were probably first given; compiled from the information shown in Appendix 1.

Session

1683-84	Optics		
1684-85	Mechanics 1 & 2		
1685-86	Mechanics 3	Astronomy 1 & 2	Practical Geometry
1686-87	Mechanics 4	Astronomy 3	Trigonometry & Logarithms
1687-88	Mechanics 5		
1688-89			} ? Hydrostatics ?
1689-90		Astronomy 4	
1690-91		Horolographia	

Chapter 2

Appendix 3

Education Papers

1. S.U.L. QA33 G8D1 Edinburgh inaugural speech. 10.12.1683.
2. A.U.L. 2206/8 Oxford inaugural speech. 21.4.1692
published in P.D. Laurence and A.G. Molland
'David Gregory's Inaugural Lecture at
Oxford' Notes and Records of the Royal
Society of London 25 (1966). pp 108-143.
3. B17: James Gregory's Inaugural speech at Edinburgh
1692.
4. C215: 'To the Committee of Parliament for visiting
schools and colleges', 1687.
5. RG fo.85 C112 'Methodus quae Mathematicis addiscendis
utilissima et facilima videtur' 16.12.1693.
Also, another copy in Pringle's notebook,
Dc.6.12, entitled 'De ratione studii
mathematici consilium'.
6. RG fo. 90. Submission of Gregory and Wallis on the
teaching of mathematics at Christ's Hospital.
11.6.1694.
7. A68 'Ordo in Math: docenda observandus a Prael:
in Coll: Ball: 1697'
8. Gregory's "collegia" scheme.
Published in Tanner (ed) The Private
Correspondence and Miscellaneous Papers of
Samuel Pepys, 1679-1703, 2 vols. (London, 1962)
2 91-94; Charlett to Pepys 15.10.1700.
Also published in T.W. Jackson 'Dr. Wallis'
letter against Mr. Maidwell' Oxford Historical
Society 5, 1885, Collectanea 1 pt. VI pp. 269-
337.

Chapter 3 Mathematics at Edinburgh

Gregory's time at Edinburgh was not all taken up by his lecturing duties. He published two works on infinite series methods of integration; the first, the Exercitatio reproduced the methods of his uncle, James Gregorie, and the 'second method', published in Pitcairne's Solutio, examined a case where the series becomes finite. (Appendix 1, Chapter 1 gives details of these publications).

To see what Gregory did in the Exercitatio we must look first at James' work and the sources from which it can be studied. However, Isaac Newton was also producing very similar results, though often by different methods. These had been set out in the tract De Analysi and in two letters to Leibniz in more systematic form than Gregorie had given any of his results.

David, perhaps with some initial help from his father, set about rediscovering his uncle's methods. This was an enormously difficult task, for James had died young and suddenly, and made no attempt to set his papers in order. Unfortunately, there are very few dates on the sheets on which David worked his reconstruction, but it is highly unlikely that much, if anything, was done before his return from London in 1681.

Now, although Newton's methods were unpublished, the De Analysi and the letters had been seen by several members of the Royal Society, including John Collins, James Gregorie's correspondent. David's trip to London certainly gave him the opportunity to discover something about Newton's work. Indeed, among his preliminary papers for the Exercitatio he mentions Newton's methods, but again the comments are

undated and it is possible that they were added after 1685, when Wallis's Algebra appeared, with extracts from the Leibniz letters.

The methods which Gregory uses in the work are more akin to Newton's than James Gregorie's. His somewhat distrustful attitude towards the binomial theorem mirrors Newton's as does his use of general index notation of powers, although this might be a genuine extension of Wallis' usage. The last three examples especially, where he used Newton's method of resolving equations into infinite series, give a different treatment of the problems from his uncle's.

In the end, though, we cannot be sure that Gregory had seen and used Newton's results. He had opportunity and motive, and a salve to his conscience in that he apparently never doubted that his uncle had used these self-same methods. As he claimed nothing in the work for his own he might have felt quite justified in using some external help in his reconstruction. The mention in his notes of Newton's method, though, and the similarities of the methods he used to Newton's work, are no more than suggestive. He may have found Newton's reduction of equations to infinite series in an entirely independent discovery. While it is tempting to conclude that the Exercitatio was built on Newton's work, and this conclusion fits what we know of Gregory's mathematical abilities, we cannot be certain on this point.

Meanwhile, though, Gregory's work for the Exercitatio had led him to examine Sluse's method of tangents. He produced a paper on this in or about the summer of 1683, and submitted it to the Royal Society. However, the paper was not an impressive one and both the Society, and Colin Campbell to whom he later sent it, quickly forgot it.

In 1684, the Exercitatio appeared. However he had derived those methods he did display as his uncle's, David did not fully appreciate

all James had done. We have seen that he did not use the binomial theorem or the so-called 'Taylor's' theorem, by which James had deduced the three examples David closes the work with. Further, he had by no means matched James' rigour in determining questions of convergence in infinite series. Dr. Whiteside's analysis of seventeenth century mathematics indicates just such a drop in earlier standards of rigour towards the end of the century¹.

Another omission from the work is an acknowledgement of the help which Gregory received from the works of John Wallis. This is especially noticeable in his treatment of the centre of gravity of a spheroidal segment.

The work began with an historical introduction which described his uncle's mathematics and mentioned that of Mercator and Isaac Newton. The latter's work was again discussed in the conclusion. Gregory's basic lemma, equivalent to $\int_0^x x^r dx = 1/r+1 X^{r+1}$, came from Mercator. He looked at hyperbolic areas, where he introduced the term 'plusquam infinitas'. His understanding of limits of integration was slight, a problem which would later lead to more confusion. He introduced rectification, and so measured the surface areas of solids of revolution.

Then David passed to the important part of the book; the production of infinite series and their term by term integration, a process which he assumed to be valid. The series were produced by three methods - division, extraction of roots and solution of equations. In the first two cases David used mechanical methods, as Newton had,

¹ D.T. Whiteside 'Patterns of mathematical thought in the later seventeenth century' Archives for the history of the exact sciences. I (1961) 179-388 262.

instead of the binomial theorem his uncle had employed. In the third case he used a totally different procedure from his uncle's. However, he reproduced many of his uncle's results in this way (if not his methods) and, in the first two cases, at least, his approaches to the problem were very similar. His calculation of the elliptic arc illustrates this point.

Two further points arise out of the work. Firstly, David's study of the logarithmic curve and its rectification, which he would look at again in his 1694 tract 'Fluxions', shows something of his concept of this function. Finally, his treatment of the function $\tan^{-1}x$ is an early example of the basic flaw in Gregory's mathematics. If he was sure a result was true, he would be totally uncritical of the methods he used to achieve it.

When Newton received the copy of the work which Gregory sent him, he was prompted to write a paper of his own to forestall any further publications of Gregory's. Otherwise, although it was used and read by several mathematicians, the work created little stir. The review of it in the Transactions was appreciative, but unexcited, and the book was swiftly overshadowed by the work of Newton and Leibniz.

However, one curious circumstance did bring the book back into the public eye. A controversy blew up between Newton and Leibniz in the early eighteenth century over priority in developing the calculus. One point of contention was the infinite series for π , derived from James Gregorie's for $\tan^{-1}x$. Gregory's Exercitatio described the series as Leibniz's, and this fact was not forgotten by the continental mathematicians.

In 1688, Gregory's 'second method' appeared, and, apart from its inclusion of a constant of integration, was Newton's 'abrupt series' of the Epistola Posterior to Leibniz. John Craige, a young Scottish mathematician, had visited Newton at Cambridge and, on his return to Edinburgh in 1685, had become friendly with Gregory and Pitcairne. In a book published thirty years later he claimed that Gregory had only developed his 'second method' from hints Craige had dropped of Newton's methods. This claim appears to be borne out by the evidence.

Gregory later said that one of the examples in his Exercitatio, written before he met Craige, was a case of his 'second method' and so, by implication, Craige's claim was false. However, although this example can be so regarded, it is clear from his notes and his letters to Campbell that Gregory did not then treat it as such.

The examples which Gregory sent Campbell of his 'second method' were those Newton had given Craige. The very way in which he developed his method shows that he knew in advance the sort of result to expect. Undoubtedly, Gregory's 'second method' was based on Craige's hints. Indeed, a paper of his dated 1686 refers to it as the Newtonian canon.

Even then, however, it took Gregory three years to develop the method to his satisfaction. A particular problem arose through his lack of understanding of the difference between the definite and indefinite integrals. He introduced a constant of integration into his formula, but did so only to bring it into line with his previous results, without appreciating its significance. John Craige, who had discussed this point with Newton, had a much fuller comprehension of it. Gregory also gradually extended his investigation to include other cases, and continued to do so even after the publication in Pitcairne's Solutio.

Meanwhile Gregory had also been studying Craige's work, which was generally concerned with algebraic extensions of Barrow's geometric theorems on the calculus. This work shows how Gregory's infinite series were only one of many approaches to the same problems. His comments on Craige's work show that his concern was not with the methods themselves, but with the results achieved, and with the superiority of his method as measured by results. However he could detect a basic flaw in part of Craige's treatment. This study also gave some stimulus to his work on the 'second method'.

The relationship between Craige and Gregory, which led to the former's angry charges of plagiarism, is hard to evaluate. However, it seems that within a few years it had changed from friendship to rivalry. Gregory gleefully challenged Craige to equal his results in the quadrature of curves, while Craige withheld his superior understanding of limits of integration. On the appearance of the Solutio Craige wrote at once to Newton for a copy of his method, and found that the two were indeed one. He wrote to Colin Campbell telling him this and, at some time in the next years, to Isaac Newton also.

The two Scots kept up a correspondence of some sort, but we have only one letter of Craige's and a mention of one from Gregory. By 1703 Pitcairne could tell Campbell that these two were far from friends.

In 1691, Gregory sent Newton, who was then helping him to win the Savilian chair, a copy of his 'second method'. He hoped to publish it in the Transactions along with Newton's method, and whatever Newton wished to say on his priority. Newton did not, apparently, reply to this letter, but, as he had done when he received the Exercitatio, set about writing a tract on his own methods. The first draft of this

tract made his opinions on Gregory's behaviour quite clear, but he had mollified these by the final version. However, this incident probably prompted the silence which followed between Newton and Gregory over the next $2\frac{1}{2}$ years.

However, Wallis published the two methods in the 1693 volume of his Opera. He presented Gregory's as an independent discovery, and suggested that he and Pitcairne had only known of Newton's work when Craige received a copy of it after the Solutio had been published. Anyway it was clearly only a first case of Newton's more general method.

In 1693, too, Gregory visited Holland where he met Huygens with whom he discussed the method. On his return he sent the Dutchman his own and Newton's methods, which caused some interest on the continent.

Although generally regarded as his own work, however, Gregory's 'second method', the first case of a general theorem of Newton's, caused no excitement and was soon forgotten by all but John Craige.

3.1 James Gregorie and Isaac Newton

In the late 1660's and the 1670's both these men were dealing with similar problems in calculus. They were producing infinite series solutions to a wide range of problems. Others, too, had used infinite series. Mercator's series expansion for $\log(1+x)$ appeared in 1668 and was rigorously proven by James Gregorie in 1669². Wallis had considered the limits of geometric series in his Mathesis Universalis and used the results in his Arithmetica Infinitorum. David was certainly familiar with all these sources as a young man, and may have known also the work of Brouncker and Mengoli.

However, Newton and Gregorie had developed techniques far in advance of the others. From 1668 until his death in 1675, Gregorie had communicated with John Collins, sending him many of his results, but almost nothing on his methods. Much of his calculations were performed on the blank spaces of letters from Collins and others, and from these the late Professor Turnbull reconstructed the chief of his methods³. We are concerned especially here with his knowledge of Taylor's theorem and of the binomial theorem.

Gregorie probably derived both of these from his finite difference interpolation formula $f(x_0+h) = f(x_0) + \binom{h}{1} \Delta^1 f(x_0) + \binom{h}{2} \Delta^2 f(x_0) + \dots$. The Taylor expansion $f(x_0+h) = f(x_0) + hf'(x_0) + h^2/2! f''(x_0) + \dots$ may be regarded as a limit case of it. This formula enables us, by

²Nicolas Mercator Logarithmotechnia sive methodus construendi logarithmos (London, 1668). James Gregorie Exercitationes Geometricae (London, 1668).

³GTV passim, but especially 347-60, 370-1.

repeated differentiation, to express an intractable expression, say $\sin x$, as a power series in x . In particular, James used this method to give an equation for the cycloid and so solved Kepler's problem, which is examined in more detail below.

From this expansion, James had developed a method of expressing the root of an equation as an infinite series. We do not know exactly what this method was, but it certainly involved repeated differentiation⁴.

Further, he had deduced from his finite difference interpolation formula a general logarithmic form of the binomial theorem,

$$\begin{aligned} \log b + a/c [\log(b+d) - \log(b)] & \{= \log b(1 + d/b)^{a/c}\} \\ & = \log [b \sum_{i=1}^n \left[\binom{a/c}{i} (d/b)^i \right] \end{aligned}$$

James sent the theorem to Collins in this form, but did not state it in the letters in the more familiar form

$$(a+b)^r = \sum_{r=0}^n \binom{n}{r} a^r b^{n-r} \quad 5.$$

However, he used it to calculate the coefficients of $(1+x)^{\frac{1}{2}}$ on at least one occasion⁶. He seems never to have stated the Taylor series explicitly, but he used it in very many different examples.

Newton had not only made his results public but had also set out several of his methods. His tract De Analysi probably written in 1669 was presented to Barrow, who lent it to Collins⁷. Thus James Gregorie had learnt a few of Newton's results. After James' death, in

⁴e.g. GTV 229 n 5.

⁵GTV 131-2

⁶GTV 370

⁷MP II 2,3 206-247.

1676, Newton wrote two letters to Leibniz, the Epistolae Prior and Posterior⁸.

The De Analysisi began with a rule equivalent to $\int_0^x at^{m/n} dt = na/(m+n) x^{(m+n)/n}$. Then followed three ways of converting expressions into infinite equations so that this rule could be applied to each term in succession. These were division, root extraction and expressing the root of an equation as an infinite series. The first two operations were performed mechanically, by following the normal arithmetic rules, and not by the binomial theorem. The theorem was used implicitly in Newton's proof of the basic rule, but for integral index only.

The Epistola Prior began by stating the binomial theorem in the form $(P+PQ)^{m/n} = P^{m/n} + \frac{m}{n} AQ + \frac{m-n}{2n} BQ + \frac{m-2n}{3n} CQ + \dots$ where A, B, C etc. refer to the preceding term. Newton explained his use of fractional and negative indices and went on to give examples of the use of this theorem. Next he gave in detail his method for reducing the root of an equation to an infinite series, which he had discussed in the De Analysisi. Given a polynomial $\sum_{r=0}^n a_r x^r = 0$, we select an approximation λ to one of the roots and substitute $\lambda + p$ for x to obtain $\sum_{r=0}^n b_r p^r = 0$. However, since λ was very close to a root, p is very small and $b_0 + b_1 p \approx 0$, $p \approx -b_0/b_1$. We continue the process by substituting $-b_0/b_1 + q$ for p in $\sum_{r=0}^n b_r p^r$, and then approximate q in the same way. For a numerical equation this gives

⁸1 Isaac Newton 'Epistola Prior' Newton to Oldenburg, 13th June, 1676 NC II 20-32.

2 Isaac Newton 'Epistola Posterior' Newton to Oldenburg, 24th Oct., 1676 NCII 110-129.

Turnbull's translations follow on pp. 32-41 and 130-149 respectively and my quotations throughout are taken from these.

an ever closer approximation to the root. When the equation involves two unknowns, the process of selecting the appropriate approximation becomes more complicated (involving Newton's 'parallelogram rule') but the basic procedure is the same. We then find one of the unknowns as an infinite series in powers of the other. The letter finishes with further examples, including Kepler's problem, the rectification of the elliptical arc, the volume of a second spheroidal segment and the rectification and quadrature of the quadratrix.

The Epistola Posterior was sent four months later, and opened with an account of Newton's derivation of the binomial theorem from Wallis's interpolation. Next he gave an example showing the expansion of $(1-x^2)^{\frac{1}{2}}$ by arithmetical means, and discussed his preference for this procedure over the direct use of the binomial. Reduction by division (as Mercator had published) was also mentioned here. A method for calculating logarithms followed and, after a description of his achievements in the De Analysi, an indecipherable anagram containing the basic theorem of the calculus - the inverse nature of the operations of differentiation and integration. The following topic, Newton's 'abrupt' series, is discussed below in the context of David Gregory's 'second method'. Next Newton gave the length of a cissoid curve, and hinted at various of his general results, including the classification of curves and his interpolative methods. Results on the circle, on hyperbolic logarithms and on the construction of trigonometric tables were followed by the 'parallelogram rule' for choosing approximations when expanding roots of equations in infinite series. Finally he gave a method for the reversal of series, and two general theorems for so doing, ending with another anagram on the inverse nature of the two methods of the calculus.

So, by the late 1670's, when David entered the picture, matters were so. Both Newton and James Gregorie had discovered similar methods and had applied them to the same problems. However, the methods were not the same and this is especially apparent in two points; the binomial theorem and the reduction of an equation to an infinite series. James had derived the binomial theorem rigorously, and had no qualms about its use for fractional or negative index. There is no sign that he ever used the equivalent arithmetical procedures. Newton had developed the binomial theorem from a study of Wallis's interpolations, and had no rigorous proof. He preferred to use arithmetical methods for negative or fractional indices, trusting the theorem only when he was dealing with positive integral index. We do not know how James turned an equation to an infinite series, but we know his method was one of repeated differentiation, which Newton's was certainly not.

James' methods were scattered on the spaces on old letters, and were in any case little more than his own private calculations. Newton had written out his methods on three occasions, and although these were unpublished, John Collins had copies of them all. There were other differences too; James, for instance, seems never to have written a fractional or negative index, using traditional and clumsier notations. Newton used these indices freely.

This was the situation when David began to examine his uncle's papers.

3.2 Preparing the Exercitatio

James Gregorie died suddenly in October 1675, not long after David had completed his four years of study at Marischal College, in June of that year. His papers and books went to his brother, Gregorie of Kinnairdie, David's father. There were soon plans that James' manuscripts should be published. Collins wrote to Kinnairdie in August, 1676, suggesting that

'as to those remains you have if you cannot get them published in Scotland, you might perchance doe well to commit them to the care of the Royall Society here'⁹.

Two months later Newton wrote to Oldenburg for Leibniz, and mentioned a supposed treatise by James Gregorie on integration by infinite series.

'which we hope is going to be published by his friends'¹⁰.

Unfortunately, there was no such tract, but merely scraps of calculations and stray results. Most of the papers which James left have since disappeared, but David catalogued them in folio D and his other files. Professor Turnbull has analyzed the papers we have and considered the titles of those which are now lost¹¹. We can see at once from the titles that there was much on Diophantine equations, on trigonometry and on elementary geometry, but there was very little on what Collins and others hoped would be published - James' methods of infinite series. There was a paper on Wallis's De Cycloide, now missing, which probably had a bearing on Kepler's problem¹².

⁹John Collins to Gregorie of Kinnairdie 11th Aug., 1676 GTU 344.5.

¹⁰Newton op cit (82) p.133.

¹¹GTV esp. 25-44.

¹²D22.

James used a Taylor expansion in this problem, which is examined below. Some of his series, generally also derived by a Taylor expansion, were given at the end of his trigonometry tracts, but with no hint of their derivation¹³. Saunders wrote a summary of James' papers not long after his death, and said of the notes which Gregorie had hoped would complete 'the Analyticks', that

'he himself acknowledged them to be all lost work, for non understand what he meant by such shorts nots, neither could he himself by all likelihood if in the same condition they are now; they had been out of his sight but half a year'¹⁴,

Faced with these notes, Kinnairdie apparently made no attempt to publish them, nor did he send them to the Royal Society. His eldest son, our David, had left University, perhaps with a bent for mathematics already apparent. Kinnairdie handed the manuscripts to him.

However, David's mathematics was not yet mature enough to tackle these papers, and we have no sign that he even attempted to do so before 1682. His father sent him abroad in 1680, where he studied the continental mathematicians of the Cartesian school. By the time he set to work seriously on his uncle's papers, he had read James' published works on mathematics, and had studied the works of John Wallis. He also knew Mercator's Logarithmotechnia, Viète's Exegetices, Sluse's Miscellanies and Isaac Barrow's Lectiones Geometricae¹⁵.

¹³e.g. C186

¹⁴E.U.L. Dc 1.41.29.

¹⁵See C196, which contains David's preliminary work for the Exercitatio.

Although Heuraet's methods of rectification, Mercator's 'long division' into infinite series and many points in Wallis' work were of use to him, none of these authors could help David in reconstructing other aspects of James' work, such as the binomial theorem and the Taylor expansion. Mechanical square root extraction he might have been able to deduce by analogy with Mercator's mechanical long division, so avoiding the use of the binomial theorem, but many problems were still left unresolved. In the summer of 1681, David visited London, and he must have been already wondering about these hidden methods James had used.

3.3 Trip to London and Newton's Methods

We know certain things about David Gregory's visit to London in May and June, 1681. He saw many curiosities, including Boyle's pneumatic pump, and Newton's reflecting telescope at Gresham College. On 4th May he attended a meeting of the Royal Society¹⁶. However, we know very little of whom he met there, and the suggestions I am making in this section can only be conjectural. Nevertheless, all the evidence seems to indicate that on this visit, probably through John Collins, he learnt something of Newton's methods of infinite series.

One thing David is almost certain to have copied when in London. His B1 is now missing, but the index describes it as 'Extracts of Mr. James Gregory's letters to Mr. John Collins, written by Mr. Collins and given to the Royal Society, anno. 1676. Of which there is a particular index'. These extracts are referred to several times in David's preliminary notes for the Exercitatio¹⁷. Of course, this may have been a copy sent to Kinnairdie by Collins in 1676, but the letter in which the Englishman explained his intention to leave these extracts with the Royal Society mentions no such intention. Instead he says that the extracts will lie there

'where any friend of his may peruse the same or have it transcribed'¹⁸.

The paper still lies among those collected together for the Commercium Epistolicum¹⁹. It seems most likely that David's copy was made in the summer of 1681.

¹⁶See chap 1.2.

¹⁷e.g. C196.20.

¹⁸John Collins to Gregorie of Kinnairdie 11th Aug., 1676 GTU 344.5.

¹⁹R.S. Cm.31.

Now, John Collins had copies of Newton's De Analysi, and of his Epistolae Prior and Posterior. Nor was he particularly secretive about them; in 1677 he sent a transcript of the De Analysi to Wallis and in 1676 he had sent some of Newton's papers on infinite series to Thomas Baker²⁰. Probably many of the Royal Society had some idea what methods Newton had set out in these papers.

Collins had corresponded with David's uncle and, on his death, written to David's father. David was busy copying the extracts he had made. It seems highly likely that David would call on John Collins when he was in London, and that their talk would concern James Gregorie's work. The similar methods of Isaac Newton, mentioned in the letters of Collins to James, seem to constitute another obvious topic of conversation. Moreover, if Collins told David nothing of Newton's work, there were others in London who could. David, with entry to Gresham College and, more especially, the Royal Society, must have known some of these men.

However, opportunity to do so is far from proof that David learnt of Newton's infinite series methods when he was in London. For further evidence we must look at the preliminary notes he made for the Exercitatio and at that work itself.

3.3.1 Mention of Newton's method

The notes from which Gregory compiled this work are mostly grouped together as his C196. Unfortunately few of them are dated and some at least have been added after the book was published. It is even possible that remarks were added later to sheets themselves written before the book appeared. We have an 'ordo faciendorum' followed by

²⁰MP II p 207 n2. Collins to Baker 19th Aug., 1676 S.P. Rigaud Correspondence of Scientific Men 2 vols (Oxford, 1841) ii 4-10

a list of 'desiderata' and many problems later inserted in the book with some miscellaneous items. Newton's method is mentioned on five occasions and an example on the cissoid shows an acquaintance with a result of the Epistola Posterior. There is further evidence in the book itself; the use of general indices, the omission of the binomial theorem and the method used to resolve equations into infinite series or to invert the series.

Of the five remarks in the notes, two occur among the 'Desiderata' and the others in a paper on rectification²¹. On the first of these we have

'Desired that proposition 16 Logarithmotechnia [the equivalent of $\int_0^x t^n dt = 1/n+1 x^{n+1}$] be briefly and clearly proven, and extended to any power, also those whose exponents are fractional or negative',

to which Gregory has added

'It is done, and also the canon for quadrature proven
16th August, 1682, but from propositions 64 and 102 of
Wallis's Arithmetica Infinitorum'

and, perhaps later again,

'or by Newton in infinite series'.

On the same sheet he wrote

ii 'Whether Wallis's interpolations can be conveniently and succinctly done by this method'

to which he added

'To this end see Newton's letter to Oldenburg'.

²¹Desiderata C196.2 Rectification C196.17. All quotations in my translation from Gregory's Latin.

The paper on rectification contains several different results and theorems, written down at odd times. Some were used in the Exercitatio, but we cannot date the whole sheet before that work was published. Here Gregory finds a series for the arc of a circle by dividing a series expression for the corresponding segment by $r/2$. He notes of the resulting series that

- iii 'the same quantity is found on operating by the common form according to Newton's method'.

The moment from which the length of a semi-cubical parabola, $ay^2 = x^3$, is calculated is, $\sqrt{(1 + 9x/4a)}$ and

- iv 'it is to be seen how the length of the curve is investigated hence by Newton's method'.

Finally, the moment corresponding to the rectification of the curve $ay^4 = x^5$ will be $\sqrt{(1 + 25/16 \sqrt{x/a})}$, and

- v 'This quantity can be handled by the method of Newton's letter to Oldenburg'.

The De Analysi supplies the proof mentioned in (i), at least for fractional indices. Wallis's interpolations (ii), are discussed in the Epistola Posterior and the series (iii) is given in the Epistola Prior. The extraction of roots, (iv) and (v) is discussed in both letters, or these comments could apply to the 'abrupt series' of the Epistola Posterior. Alternatively, remarks ii to v could refer to Wallis's Algebra of 1685 which gave the binomial theorem as Newton had written it to Leibniz²².

On another sheet among the preparations for the Exercitatio, there is an attempt on the rectification of the cissoid²³. Gregory's attempt to rectify the curve directly failed when he reached an impossibly

²²John Wallis A treatise of algebra, both historical and practical (London, 1685) 318-20, 330-47.

²³Cissoid Cl96.25.

complex expression, but he devoted the rest of the sheet to a study of the area between a hyperbolic curve, its axis and two tangents to it. But Newton's Epistola Posterior had related this rectification directly to this area. Clearly Gregory was attempting here to reconstruct a result of Newton's. This example is not in the Algebra, but perhaps John Craige told Gregory of it in 1685.

These comments are far from conclusive. Gregory may have added them all after the work was published, when he read Wallis's Algebra, a work which he had received by February, 1686²⁴. At around this time he also copied the tract De Seriebus Infinitis from John Craige who had made these notes on Newton's 1671 tract on fluxions²⁵.

3.3.2 Indices and the binomial theorem

In the Exercitatio itself we find more evidence. Gregory's use of indices in the book was freer than in any previous published source. He used, and explained the use of, both fractional and negative indices in a manner which James Gregorie never had. This use had been implicit in Wallis, but Cajori found that they were first made public in the Epistola Prior²⁶. The justification Gregory makes for this use is almost certainly a conscious development of Wallis's arguments, and he may have developed the notation for himself after studying Wallis. However, this notation makes noticeably freer use of negative and fractional indices after his return to Scotland in 1681, and it may be that this notation was another thing David learnt in London, perhaps again, via Collins from Newton.

²⁴Gregory to Campbell 25/2/1686 CCC.

²⁵A 56. MP III 1 2 Appendix 354-72.

²⁶Florian Cajori A History of mathematical notation 2 vols. (Chicago, 1928) ii 356.

The binomial theorem provides another puzzle about the Exercitatio. David apparently knew its form, and its validity for fractional and negative index, but he did not use it in the book.

His 'Ordo Faciendorum' stated

'The most beautiful rule for removing asymetries is to be given, and proven from Wallis's Arithmetica Infinitorum or otherwise'²⁷.

The arithmetical method which Gregory used in his Exercitatio for 'removing asymetries', or reducing a square root to an infinite series, could hardly be regarded as a 'most beautiful rule'! Nor would Gregory have considered it needed proof. This can only refer to the binomial expansion for fractional index, and its inclusion in the 'Ordo Faciendorum' shows that this note was made before 1684, while David was planning and writing the book.

Secondly the 'Desiderata', again in the main body of the paper and not as a later addition says

'That the method of finding the coefficients of the terms of $\frac{1}{a+b}^{p/r}$ is proven independently, from Wallis or some other, and that the nature of any term of the series when the exponent is a fractional or negative number be more clearly established'²⁸.

Even more clearly we can see here that Gregory knew the binomial theorem for positive integral index and its validity for other indices, although a little unsure of the form the series took in these other cases.

²⁷C196.1

²⁸C196.2

Another sheet examines the surface of revolution of a parabolic conoid which appears in the Exercitatio. On this the binomial coefficients are written out for positive integral index, m , as $1, m$, the triangle of $m-1$, the pyramid of $m-2$ and so on. This, says Gregory, is clear from the table on page 12 of Mercator's Logarithmotechnia, which derives the figure's we now know as 'Pascal's triangle' in a different context. Some other, less reliably dated, sheets in this collection mention the binomial coefficients, but only these mention any sort of source.

It seems unlikely that Mercator, who only mentions briefly that the numbers he gives are the coefficients for positive integral index, was David's source. There are three other possibilities; he may, as Newton had done, have derived the expansion for himself from Wallis's interpolations and inspection of his mechanically derived series, or he may have known of it directly from his uncle's work or indirectly from Newton's.

Had he derived it for himself, it seems highly unlikely, and out of character, that he would then omit it from the Exercitatio. We would also expect that such a derivation, even if only by inspection, would have led to some sort of proof, or justification for its use. Of course, it is possible that he found the rule for himself, but decided not to use it and never in later life referred to his independent discovery of this important theorem. However, the second two possibilities seem more likely.

As explained in 3.1, James Gregorie had stated the binomial theorem in a disguised form as $\log b + a/c [\log(b+d) - \log(b)] = \log [b \sum_{i=1}^n [(\frac{a}{c})^i (d/b)^i]]$. Collins sent him Newton's series for

the zone of a circle, which had been derived by expanding $2(R^2 - x^2)^{\frac{1}{2}}$ as an infinite series and integrating term by term, but for many months James was unable to reproduce this result. When he finally saw that it followed from this series of his for finding a number corresponding to a given logarithm, he wrote to Collins.

'I admire much my own dulness that in such a considerable time I had not taken notice of this'²⁹.

In the Exercitatio, David related that James received Newton's series and replied to Collins that it followed from his own rule for finding a number from a given logarithm, or changing a root into an infinite series³⁰. Later in the work, David derived this series from a process of mechanical extraction³¹. It has naturally been assumed from this that David knew the binomial theorem could be derived from his uncle's logarithmic series³².

In fact, we have no evidence of this. His comment about the logarithmic series in the introduction to his work is merely quoted from James' letter to Collins and need not imply that David knew how his uncle had derived one series from the other. The preliminary notes for the Exercitatio do not mention this logarithmic series, nor make any attempt to use it. Moreover, had David so derived the binomial theorem he would have had no need of a proof. Nor, if he even suspected that the theorem could be so derived, would he have looked in Wallis for a proof. The Arithmetica, which Gregory mentions, does not discuss logarithms and, while it might have been generally useful,

²⁹James Gregorie to John Collins 19th Dec.1670 GTV 148

³⁰Exercitatio 3

³¹ibid 22

³²See, for example, Whiteside op cit (1) 260.

there was no reason to look there first for proof. The only possibility is that James had written down the binomial coefficients, separately from the logarithmic series, in a paper which is now lost. David, on finding this, might have turned to Wallis for a proof.

On the other hand, Newton's Epistola Posterior mentions Wallis's interpolations as the source from which he derived the theorem. If David learnt the form of the coefficients from Collins, or another who knew them from reading Newton, it would be reasonable to expect him to know also the source from which they were derived. While the alternatives are by no means impossible, it seems most likely that David heard of the binomial theorem in London.

This may also explain why David did not use the theorem in the Exercitatio. Of course it may simply have been that he was reluctant to use it without proof, when mechanical division and root extraction were sufficient for the problems he tackled. Yet even this is reminiscent of Newton's attitude to the theorem.

However, if he learnt of the theorem in London, and could not see the connection with his uncle's logarithmic series, there was every reason not to publish it. He knew of Wallis's intention to publish Newton's method of expressing the roots of equations as infinite series, and may also have known that the binomial theorem would form a large part of the Newtonian extracts which Wallis would publish³³. Naturally he would not publish the binomial expansion himself under these circumstances, but if he had found it in his uncle's notes, or developed it himself, suspicion of Newton's forthcoming publication of it would surely have urged him to print it, even without proof, to establish a claim on his own or his uncle's behalf.

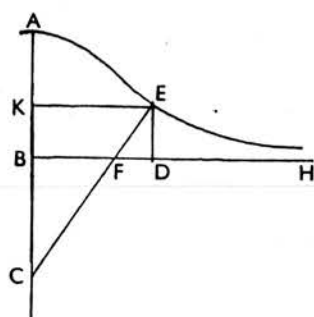
³³Exercitatio 48

Thus, the binomial theorem and David's treatment of it suggest again that he had learnt something of Newton's methods in London. This appears too in his resolution of the roots of equations into infinite series.

3.3.3 Roots of equations as infinite series

As we saw in 3.1, Newton's letters to Leibniz described a method of displaying the root of an equation as an infinite series. The last three examples in the Exercitatio employ an unstated rule for performing this, or an allied, operation. On examination of Gregory's preparatory notes we find that the method was precisely Newton's.

The first of these three examples was the conchoid of Nicomedis.



Given the points A, the vertex, and C, the pole, and the norm BH intersecting AC at B, the conchoid is traced out by E, the end of CE rotating about C, meeting BH at F, such that $EF = AB$. Gregory takes the case where $AB = BC = a$.

In his draft notes, Gregory took DE as y and BD as x , to derive the equation $y^4 + 2ay^3 + x^2y^2 - 2a^3y - a^4 = 0^{34}$. He was unable to proceed from there, and tried a second approach with AK as x and KE as y . This gave an infinite series and the area of the conchoid, which he gave on pages 24,5 of the Exercitatio.

However, he did not forget the alternative formulation, but returned to it and expressed it as $y = a - x^2/8a + x^4/128a^3 + 3x^6/2048a^5^{35}$.

³⁴C196.11

³⁵C196.27

To find this result he used Newton's method, not only in general principle, but (with the trifling change of Newton's $p, q, r \dots$ to his own $k, l, m \dots$) in every detail of layout and calculation.

This result, too, he gave in the *Exercitatio*, but with no details of method. He described his procedure generally as

'by extracting the roots of equations in species, almost in the manner of Viète's Numerosae Exegetices, which resolution of equations is also necessary when the base is required in turn from a given area'³⁶.

Either David had deduced this procedure from Viète, coincidentally finding precisely the form and layout Newton had, or the method had been suggested to him in London.

In the second example, David finds the base corresponding to a given hyperbolic area. Newton's Epistola Posterior had pointed out that his method of reducing equations to infinite series could be used in this problem, although he did not give an example of this. Of course, it is more directly performed by the method of reversal of series; that is, by changing a series expressing y in powers of x (the area as powers of the base here) to one expressing x in powers of y . However, by taking a finite number of terms of the initial series, the above method is quite adequate. Gregory does not give his method, nor do we have his preliminary notes, but as he used this procedure in the third of our examples it seems probable that he used it here too. Even if he did use series reversal, the procedure for this is given clearly in Newton's Epistola Posterior, but is not stated in James Gregorie's papers.

Again, this was a problem which David had not at first been able to solve. He noted among his 'Desiderata'

³⁶Exercitatio 46

'4. I am afraid that finding the base from a given area cannot be done or treated by a known method of resolving equations'³⁷.

He later resolved below this note to see if the work of Oughtred, Viète or Harriot could help him here. As with the conchoid, he may have derived such a method from these sources, or he may have been able to perform this operation only after hearing of Newton's work.

The final example in the book was Kepler's problem. This arose out of Keplerian astronomy and is essentially a question of finding the point which a body has reached at any given time when it is moving in the periphery of an ellipse in such a way that the radius vector joining it to one focus of the ellipse sweeps out equal areas in equal times. It is mathematically equivalent to finding the point on the circumference of a circle such that the line joining this point to a given point on the diameter will divide the semi-circle above the diameter in a given ratio. This problem had been studied throughout the seventeenth century, but James Gregorie and Newton had been the first to derive analytical solutions.

In 1659, Wallis's tract De Cycloide had contained Christopher Wren's proof that the problem could be reduced to finding where a certain line cuts a trochoid. However, the relationship between abscissa and ordinate of a trochoid cannot be expressed as a finite equation in x and y . Wren's solution could only be used in practice by actually drawing a trochoid and measuring its ordinate for a given abscissa.

Professor Turnbull has shown that James Gregorie deduced his solution of Kepler's problem by starting from Wren's work, and expressing the trochoid as an infinite series equation in x and y ³⁸. Taking

³⁷C196.2

³⁸GTV 228.9, 361-7

the parametric form of the equation to the trochoid,

$$x = r(1 - \cos \phi) + b \phi, \quad y = r \sin \phi,$$

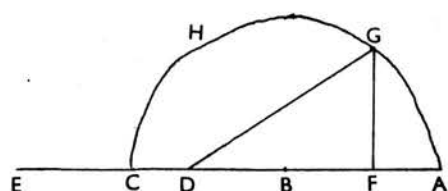
he found successive derivatives of y with respect to x , to get

$$y = rx/b - r^2 x^2/2b^3 + r^3 x^3/2b^5 - rx^3/6b^7 \dots$$

An alternative parametrisation gives rise to the alternative equation

$$e = ra^2/2b^2 + (ra^4/6b^5 - ra^4/24b^4) \\ + (ra^6/720b^6 - 13r^2a^4/320b^7 + 7r^3a^6/72b^8) + \dots$$

By applying this to Wren's result, James Gregorie was able to send Collins his solution of Kepler's problem on April 9th, 1672³⁹.



B is the centre of the semi-circle AHC

on whose diameter AC the point D is

given. We require to find G, such that

DG divides the semi-circle in the ratio p to q .

Extend AC to E, such that $BD/BC = BC/BE$ and let $AE = b$. (In this step, the derivation from the trochoid clearly influences the approach). Take m such that $BD/BC = \text{arc AHC}/m$, and let $a = pm/p+q$, and radius $AB = r$. Using Wren's result, and the formulae for the trochoid, James could say

$$AF = ra^2/2b^2 + (r^2a^4/6b^5 - ra^4/24b^4) + \\ (ra^6/720b^6 - 13r^2a^6/360b^7 + 7r^3a^6/72b^8) \dots$$

Alternatively, if D lies nearer C , and the ratio $p : q$ is greater, we may let $m/2 + r - a = e$ and $BE = d$ and use the second trochoid formulation, to get

$$BF = re/d - r^2e^2/2d^3 + (r^3e^3/2d^5 - re^3/6d^3) + \\ (7r^2e^4/24d^5 - 5r^4e^4/8d^7) \dots$$

After this solution, James remarked

³⁹ibid 227

David gives the result in the Exercitatio in the terms used by his uncle, but the method is rather that of Newton⁴³. In his published version, he gives his uncle's figure and defines b, m, r and a in the same way. Then he writes down the result that, under the conditions of the problem, where $AF = x$

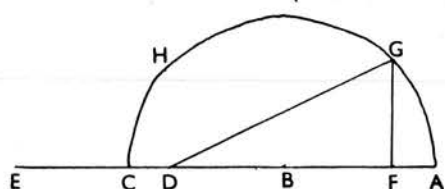
$$a = \sqrt{2br}^{-\frac{1}{2}} x^{\frac{1}{2}} - \frac{2}{3\sqrt{2}} r^{-\frac{1}{2}} x^{\frac{3}{2}} + \frac{\sqrt{2}}{12} br^{-\frac{3}{2}} x^{\frac{5}{2}} \dots \quad 1$$

and on resolution of the equation

$$x = r a^2 / 2b^2 + r^2 a^4 / 6b^5 - ra^4 / 24b^4 + ra^6 / 720b^6 \dots,$$

which is James' first answer. He does not attempt to give James' other formulation, but remarks that this series will approach the result more quickly when D lies near B and the ratio $p:q$ is less.

We have the rough paper on which David completed the preliminary drafts of this problem, and here it is even clearer that he has not followed his uncle's method⁴⁴. He refers to a 'chartula' for the



initial conditions of the problem, and this is probably the copy of James Gregorie's solution which John Collins

sent to Kinnairdie, for the initial conditions are those James gave⁴⁵.

David deduces first that $ar^2/(p-r) = 2ADG$. However, he already has a series for AFG in terms of x ; that is, for a segment of a circle given its versed sine, derived by expanding the square root of a binomial. Also, $2\Delta GFD = FD \times FG$, and $2ADG = 2\Delta AFG + 2\Delta GFD$ gives us equation 1 from the Exercitatio. Next he squares this series to get integral powers of x and to remove the surd $\sqrt{2}$. Then since he wishes the powers of a in the expansion of x in terms of a to ascend only to a^6 , he takes x in the expansion of a^2 only as far as x^3 , to get $a^2 = (2b^2/r)x + (b^2/3r^2 - 4b/3r)x^2 + (\frac{2}{ar} + \frac{4b^2}{45r^3} - \frac{14b}{45})x^3$.

⁴³Exercitatio 47,8

⁴⁴C196.32

⁴⁵GTV 344

Thus 'on operating by the common form'

$$x = ra^2/2b^2 + r^2a^4/6b^5 - ra^4/24b^4 + ra^6/720b^6 \dots$$

Unfortunately, his calculations of this step are missing, but his preparations for it (taking a finite expression for a^2) show that he was almost certainly using an expression to give the root of an equation as an infinite series. In this case, he would be unlikely to have used a different form from that he used for the conchoid; that is, he probably used Newton's method.

The example of Kepler's problem is typical of the book. David gave his results in the terms in which James had given them - even when these were appropriate to a totally different derivation from that which David used. His methods were generally closer to Newton's than to James', yet, although the layout of his reduction of equations to infinite series is suggestive, there is nothing which could only have come from Newton.

3.3.4 Did the Exercitatio rely on Newton's methods?

In that the methods David used in the work were the methods devised by Isaac Newton, the answer to this must be affirmative. However, we will never be sure that these methods were not independently derived, by David or his uncle.

The mention of Newton's method in the preliminary notes could have been added after David had read Wallis's Algebra or spoken to John Craige of Newton's work. His use of indices may have derived from Wallis. He may have derived the binomial theorem for himself or found it in his uncle's work, but have decided not to use it because he had no proof. He certainly found reduction to infinite series by division in Mercator,

and may have deduced the reduction by root extraction by analogy with this. The reduction of the root of an equation to infinite series may have been David's independent discovery and its similarity to Newton's merely coincidence.

On the other hand, John Collins may have shown him the letters to Leibniz and the De Analysi when he visited London.

This second alternative, though, would have given David far more information than he apparently possessed. He would, in particular, have known of Newton's 'abrupt series', discussed below, in 1681, which was simply not so. Most likely, John Collins or some of the Royal Society, knowing David's interest in these matters, gave him some broad descriptions of Newton's work with perhaps the occasional detail, and from these hints he reconstructed those methods, believing he was reconstructing his uncle's; in much the same way, in fact, as he reconstructed his 'second method of quadrature' from hints John Craige dropped about Newton's 'abrupt' series. This, as I said at the beginning of this section, can only be conjecture, but it seems probable and certainly cuts out the number of coincidences we must otherwise assume.

However, not all David's time on his return from London was spent on the Exercitatio. He also submitted his first paper to the Royal Society.

3.4 Sluse's Method of Tangents

Around 1636, Fermat had applied his method of maxima and minima to the determination of tangents, to derive a procedure equivalent to applying the formula $\frac{f(x+h) - f(x)}{h} = \frac{f(x)}{t}$, where h is an infinitesimal quantity and t is the sub-tangent. However, the basis of his method was very unclear and heavily criticised by Descartes because of this. Neither Descartes nor Fermat were able to put this method on a firm foundation, and, although it became generally accepted, it remained cumbersome to apply⁴⁶.

René François de Sluse was the first to develop a general algorithm which allowed one to pass directly from the equation to a curve to its sub-tangent⁴⁷. His method was apparently never rigorously proven, and remained unpublished until a short paper appeared in the Philosophical Transactions for January 1673⁴⁸. We do not know now where David Gregory first encountered this method, but there is still among his papers an extract from this article of Sluse's, written in an unknown hand⁴⁹. While Sluse is frequently mentioned in the Gregory-Collins correspondence, his method of tangents is never discussed in any detail, so we may assume that Gregory's knowledge of the method did not ~~come~~ come from his uncle, but from an unknown acquaintance, perhaps met on the continent in 1680 or in London in 1681, who gave him this paper.

⁴⁶See Margaret E. Baron The origins of the infinitesimal calculus (Pergamon Press, 1969) 168-70 or Carl B. Boyer The history of the calculus and its conceptual development (New York, 1959) (First published in 1949) 157-9.

⁴⁷Baron *op cit* (46) 214-7.

⁴⁸PT 8 (Jan., 1673) 5143-7.

⁴⁹A72. In 1691 Gregory was also sent de Volder's exposition of the method, A39 and C81.

The method is essentially this: given a function

$$f(x,y) = \sum a_{pq} x^p y^q = 0 \quad \text{we may take } y/t =$$

$$- \frac{\sum p a_{pq} y^q x^{p-1}}{\sum q a_{pq} x^p y^{q-1}}, \text{ or, in more familiar notation,}$$

$dy/dx = - \frac{\partial f / \partial x}{\partial f / \partial y}$ ⁵⁰. When planning his Exercitatio, one of Gregory's intentions was this;

'Sluse's rule to be proven from Fermat's method or
from Barrow Lect. X' ⁵¹

Gregory wrote such a paper, telling Colin Campbell in February, 1687 that he had done so 4 or 5 years ago⁵², which would mean at least that the paper was written before he took up the professorship at Edinburgh in autumn, 1683. This paper was, he told Campbell, sent to the Royal Society, and another copy was later sent to Campbell⁵³, but neither Colin Campbell nor the Royal Society appear to have thought enough of it to preserve it, and the paper itself no longer exists. We do, however, have Gregory's rough notes for it, and can see in what his proof consisted⁵⁴.

Taking the curve $v^m = py^s + y^n$, Gregory finds its sub-tangent ($= y/\frac{dy}{dx}$) by Fermat's method which is considered proven 'from the very method of operation which Descartes uses to illustrate the matter'.

⁵⁰Baron op cit (46) 215-6.

⁵¹C196.1.

⁵²Gregory to Campbell 2/2/1687 CCC.

⁵³Gregory to Campbell 25/2/1686 & 2/2/1687 CCC.

⁵⁴C143.

⁵⁵Exercitatio 47,8.

This gives us $mv^m = a \times psy^{s-1} + any^{n-1}$, where a is the required subtangent. This, says Gregory, is Sluse's canon, q.e.d. A similar calculation is also carried out for the function $v^m = py^s v^t - y^r$.

Gregory concludes

'In all other cases, Sluse's canon is deduced, mutatis mutandis, from Fermat's method, as it is not necessary to prove more fully here'.

Perhaps it is unfair to judge the paper itself on a rough draft. Perhaps the paper really did prove the matter 'more fully'. However, there is no indication on these sheets, which are indexed as 'D.Gs Demonstratio Methodi Slusii de Tang:', of any fuller method. Nor does the lack of response from Colin Campbell and the Royal Society suggest that the finished paper was any more impressive.

This was a poor start for a would be mathematician. Unsure of the date when it was written and of the date when the Edinburgh post was offered, we cannot tell if this paper was sent off in the hope that it would help Gregory to a chair. We can, though, be sure that it was intended to establish him in the eyes of the scientific world as a respectable mathematician.

3.5 Exercitatio Geometrica

On 29th March, 1684, Gregory's Exercitatio received its imprimatur. The work, as we have seen, purported to reconstruct James Gregorie's methods, but was perhaps more strongly influenced by the work of Isaac Newton. However, some other points in the work are worthy of notice, not least the omissions from it. Convergence considerations are ignored in the work, and the considerable help David received from the work of John Wallis is not mentioned.

It is a slim volume of fifty pages, which may be divided into two parts. The first of these is an introduction giving some historical background and some basic lemmas and procedures. Secondly, infinite series are introduced, and many examples of their use given. One of these, Gregorie's series for the function $\tan^{-1}(x)$, is especially interesting.

3.5.1 Omissions from the Exercitatio

James' favourite method of series was the Taylor expansion, through which he derived many series by repeated differentiation. David never discovered this expansion and it is perhaps the most obvious lack in the work. All the results David gives were found by some integration process, whether quadrature or rectification. The approach David took to Kepler's problem (see 3.3) illustrates this point.

Today, though, however the infinite series were derived, we would expect a work devoted to them to spend a lot of time examining questions of convergence. This concept is almost totally absent from the Exercitatio. We can only find two brief remarks. David tells us

that the power series in x which he has found for the area under a conchoid will approach the required area more quickly the smaller x is. Again, after giving a similar solution to Kepler's problem he repeats James' comments on conditions for a more rapid approximation⁵⁵. Otherwise convergence is forgotten.

Discussing the discoveries of infinite series in the seventeenth century, Dr. Whiteside contrasts the situation in mid-century when few such series were known and

'serious attempts had been made to formulate the concept of sequence on a strict basis and to set up concepts of (and indeed tests for) convergence'.

with this later period. He points to

'the English mathematician who, while he could marvel (on a numerical level) at the accuracy and flexibility of the infinite sum-sequence, would be therefore largely unconcerned with such theoretical functional considerations as uniqueness, periodicity and limit convergence'⁵⁶.

James and David illustrate this point well. Professor Turnbull found that James

'had the clearest views on infinite series. He frequently shifted his axes to suit the special conditions of the general problem'⁵⁷

⁵⁵Exercitatio 47,8

⁵⁶Whiteside op cit (1) 262.

⁵⁷GTV 192 n 4.

He also gave one of the earliest examples of a comparison test for convergence and was the first to use the term 'convergent' though in a somewhat different sense from that used today⁵⁸. However, his comprehension of limits of convergence and related problems is never explicitly stated. It is generally from his careful choice of approach to a problem that we can now detect his understanding. David apparently saw nothing of this.

Partly David's cavalier attitude to convergence less than ten years after his uncle's death, simply marks him as a man of his time. However, it can perhaps also be traced to the fact that he was reproducing his uncle's (and Newton's?) results. He had no need to worry about convergence conditions, for James had already accepted the results which David put in the Exercitatio. Since he found no explicit consideration of convergence among his uncle's papers, it did not occur to him to look for such considerations made implicitly.

The other point we might be surprised not to find in the work is an acknowledgement of John Wallis. The work itself is a reconstruction of James Gregorie's work, but many concepts did arise from Wallis. David's understanding of a centre of gravity, for example, arose from his study of Wallis. His notes on finding the centre of gravity of a spheroidical segment contained two generous references to Wallis, but these were omitted from the published work⁵⁹. In other places, too, references to Wallis in the notes have been omitted from the text. Finally, Wallis was only mentioned in connection with his promise to publish something of Newton's methods⁶⁰. David

⁵⁸ibid 230 in a letter of 9/4/1672; James Gregory Vera circuli et hyperbolae quadratura (Padua, 1668) applies 'convergent' to two sequences, each converging (in our sense) to a common limit.

⁵⁹C196 20

⁶⁰Exercitatio 48

would later pay ample tribute in his 'Life of Wallis' to the value of his work, but he did not do so in the Exercitatio⁶¹.

Perhaps this was an attempt to cut printing costs by reducing the number of words. Perhaps, if he had heard something of Newton's work, David felt that if he did not mention this he could not mention Wallis either. However, neither of these reasons is especially convincing and the reasons for Gregory's decision not to record his debt to Wallis at this point must remain mysterious.

3.5.2 Introduction

I have considered as introductory the prefatory historical note and that part of the work which does not discuss infinite series. That is, the introduction considers James' work, index notation, the notion of 'elementum', the basic theorem for integrating x^n , hyperbolic areas, limits of integration, though in very little depth, and rectification.

The discussion of James' work is brief and colourless but accurate in so far as it goes. David describes the work of James' Vera Quadratura, and how Mercator's work inspired him to study infinite series further. Newton's zone series, sent to him by Collins, proved to be a particular example of his own series 'to find a number, given its logarithm'. Later, Newton would read this as acknowledging that James' work was derived from his zone series⁶².

⁶¹RG fo. 89.

⁶²MP IV 527.

After discussing negative and fractional indices, David defined an 'elementum', a concept which was important especially for the modifications it led him to make in writing of Newton's method of fluxions, ten years later. An 'elementum' is that infinitesimal amount by which a quantity (area, line, volume or surface) is increased or decreased when the corresponding base line is increased by one unit. Summing these elements gives the required total quantity. Gregory's source for this concept is not mentioned, in the text or in his notes, but it was far from uncommon. Since the elementum was measured in units of the increase in the base line, its measurement was in effect the ratio between these two quantities. However, since this was not made explicit in the definition much of the flexibility gained in considering a ratio was lost.

To sum them, Gregory stated a lemma which is essentially $\int_0^x t^n dt = 1/n+1 x^{n+1}$. We know from his notes that he based this on the similar one in Mercator's Logarithmotechnia⁶³, but it was by this time a common assumption. What was unusual, though, was that Gregory explicitly extended it to fractional indices and, in fact, used it also for negative ones.

He had intended to prove this lemma, or at least this extension of it. On August 16th, 1682, he believed he had done so from Wallis's Arithmetica Infinitorum, propositions 64 and 102 (which state the lemma for positive or negative integral index)⁶⁴. However, the Exercitatio merely states that this lemma is generally held as proven among geometers. We must assume that either he was dissatisfied with his proof, or he found that the lemma was indeed so widely accepted that it had no need of proof.

⁶³C196. 1 & 2.

⁶⁴C196. 2.

The remainder of the book is concerned with applying this lemma; first in simple finite cases, where we have only $L = \sum_{r=1}^n a_r x^r$. Almost at once Gregory runs into difficulties with the hyperbolic curve $L = A^2 x^{-1}$, for which he concludes only that the area is $1/0 A x^0$ which equals infinity. For negative areas, such as that under $L = A^3 x^{-2}$, $(-A^3 x^{-1})$, he introduced the term 'plusquam infinitas', or 'more than infinite'. John Craige would repeat this term, but neither of the men explained what they understood by a more than infinite space⁶⁵.

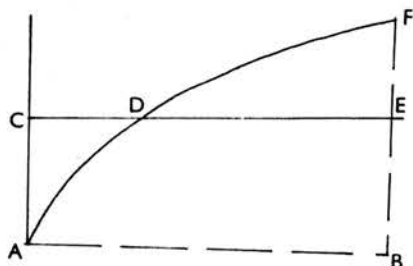
In the Exercitatio Gregory skated over the problem of limits of integration, although in his notes he had made a study of the areas represented by the indefinite integration of the parabolae $(y \pm c)(y \pm b) = p(x \pm a)$ ⁶⁶. In each case he examined the coincidence or otherwise of the area so found with the definite integral from 0 to x . Clearly, if $f(x) = 0$, then $\int f'(x) dx$ coincides with $\int_0^x f'(x) dx$, provided that we make no distinction (as Gregory made none) between the free variable x and the fixed point X on the base line. In other cases something must be added to or subtracted from the indefinite integral to find the required definite one, and Gregory discovers what these areas are in each case. He recognized that, in some sense, the indefinite integral represented all the areas to the left of the point x , but he had no knowledge of the formula $\int_a^b f'(x) dx = f(b) - f(a)$, from which definite integrals can be calculated. The insights which he gained in this study went some way towards helping him find $\int (ab + ax)^{\frac{1}{2}} dx$, of which there are two

⁶⁵Exercitatio 8; John Craige Methodus figurarum lineis rectis et curvis comprehensarum quadraturas determinandi (London, 1685) 12.

⁶⁶C196. 24.

examples in the Exercitatio. However, as this may be regarded as an example of his 'second method', its examination is deferred until 3.9.

In the Exercitatio, Gregory considered only the curve $(y+b)^2 = ax$, shown in the figure with AC as y-axis and CE as x-axis⁶⁷. The



indefinite integral here will give us DEF - DCA, not the complete area under half the parabola from the vertex to a distance x . To find this space

Gregory does not proceed as we would by taking suitable limits of integration, but suggests a change of axes, to BF and AB, thus avoiding rather than solving the problem. Otherwise problems in the Exercitatio were those where limits of integration posed no problem.

The next topic was rectification, or measuring a curved line. Here we must evaluate $\int (1 + (dy/dx)^2)^{1/2} dx$ to measure the length of $y = f(x)$. Gregory's definition was the geometrical equivalent of this analytic one and closely resembled that given by Heuraet⁶⁸. He defines an auxiliary curve whose ordinates are proportional to the 'elementa' of the curve to be rectified. The quadrature of this auxiliary curve will therefore give the rectification of the original one. With this procedure the measurement of surfaces of revolution follows. Gregory also shows how the principle of summing over the elementa applies to finding the volume of solids of revolution.

His introduction, with the exception of the negative and fractional indices, contained nothing new, but was a fair exposition of basic principles. It was the following section, on infinite series, which introduced new work.

⁶⁷ Exercitatio 8,9

⁶⁸ ibid 13,14

3.5.3. Producing and integrating infinite series

The bulk of the work considered curves whose equations were such that the above methods would not work. The principle of integrating infinite series term by term, with no regard for convergence or otherwise, was simply assumed and three methods were given for producing them.

The first, long division by a binomial (or sometimes longer, even infinite) expression was taken over from Mercator as proven in James' Exercitationes. The second, extraction of square roots, was carefully explained. On one occasion Gregory extracted a cube root by this method, leaving the obvious extension for his readers to find. (Even here he did not use the binomial expansion, as is clear from his private notes⁶⁹.) The third method was that of expanding equations into infinite series, of which Gregory gave no explanation at all. His next work, he suggested, might contain an explanation of this method, if he was not forestalled by Wallis's publication of Newton's method. Thus, the first procedure is justified by James' published proof, but the second only by analogy with numerical examples and the third not at all.

However, it may be that Gregory hoped Viète's calculation of a circular arc might prove some justification. He had already given a series based on root extraction, and gave this as an alternative approach. This calculation proceeds by repeated bisection, and James Gregorie had generalized it in a letter to Collins⁷⁰. However,

⁶⁹C196. 27.

⁷⁰GTV 68.70.

David does not give his uncle's generalization, and we can only suppose that he has inserted this somewhat irrelevant calculation out of admiration for a clever result or, perhaps, as some form of justification. It is an intuitively acceptable and rigorously proven geometric model of a circular arc as the limit of a sum sequence of straight lines. It may be that David hoped that this example would in part justify the unproven, analytically derived, sum sequence expressions among which it was placed.

In the rectification of an elliptic arc we can see how David reconstructed his uncle's work. On February 15, 1671, James had sent this series to Collins, presumably in answer to the letter of December 24 in which Collins said that Newton had rectified the ellipse⁷¹. James' letter also gives the changes necessary to accommodate the series to the hyperbolic or circular arc. David derived the series just as his uncle appears to have done, from the expansion and term by term integration of $(c^4 - c^2x^2 + r^2x^2)^{\frac{1}{2}} (c^4 - c^2x^2)^{-\frac{1}{2}}$ 72. Where James probably applied a binomial expansion to each bracket and multiplied them together, though, David extracted the square root from each bracket and divided one infinite series by the other, as can be seen from his notes⁷³. He then added James' remarks on the adaptations necessary to give the hyperbolic or circular arc.

No explicit reference is made at this point, or any other in the book, to James' letters and papers, although his published works are often cited. However, the preface had previously made it plain that

⁷¹GTV 171, 173 n 5.

⁷²Exercitatio 32 and GTU 366.

⁷³Cl96.20.

all results were to be attributed to James Gregorie, so further reference to his papers was unnecessary. In this case, and in many similar ones, David had followed his uncle in broad essentials. James' derivation was probably somewhat neater than David's division of one infinite series by another, but this was basically an unimportant difference. He copied his uncle's remarks on the correspondence with the results for hyperbola and circle, but we cannot judge how far he understood their derivation and significance.

Other examples include the area under a cycloid, the rectification of an hyperbola between oblique axes (which is taken almost word for word from one of James' letters⁷⁴), the volume of a second spheroidal segment (which Newton and James Gregorie had both discovered) and the examination of centres of gravity.

The rectification of the logarithmic curve is interesting for the geometrical interpretation which David applies to his answer. This involves integration of $(1 + r^2/x^2)^{\frac{1}{2}}$ which could be alternatively expanded as $r/x(1 + x^2/r^2)^{\frac{1}{2}}$. The second of these series gives an answer $m + (c^2 - b^2)/4r - (c^4 - b^4)/32r^3 \dots$, where c and b are the limits of integration. (David, searching only to repeat his uncle's solution, shows no sign of fully understanding the significance of using these limits). James Gregorie defines it as an ordinate of the logarithmic function, (equal to $r \log (c/b)$).

David correctly derives $m = \int_b^c r/x \, dx$, and writes this as $r/0 \cdot c^0 - r/0 \cdot b^0$. By considering these quantities as areas under a hyperbola, he relates them to the logarithmic curve, and so to the line his uncle had given. His notes used Barrow's Lectiōnes at this point, but he did not give this reference in the book⁷⁵.

⁷⁴GTV 189.

⁷⁵Exercitatio 40. Cl96.30.

The notation, $r/0 c^0$, is, of course, most unfortunate. However, in handling such amounts, Gregory treats them consistently much as we might treat $r \log c$. His use of the logarithmic function, and understanding of its connection with hyperbolae, is good. This function was a modern concept and not an easy one, yet David (with some help from his uncle's notes) could cope with it quite comfortably. He returned to this problem of rectifying the log curve in 1694, when he wrote his tract on Newton's method of fluxions, but there he followed de l'Hôpital and not his uncle.

These and similar examples, taken from various different sources but, almost invariably, previously solved by James Gregorie, illustrated the first two methods of producing infinite series. The final examples illustrating the method of reducing an equation to an infinite series, are discussed above. However one example, the series for $\tan^{-1}x$, illustrates a trait in Gregory's mathematics which would reappear several times.

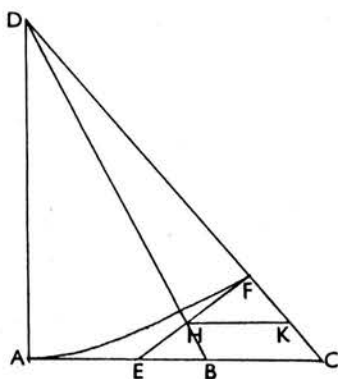
3.5.4 The series for $\tan^{-1}x$

On February 15th, 1671, James Gregorie had sent Collins a list of series corresponding to trigonometric functions⁷⁶. Most of these had been derived by a Taylor function, but one, that which gives the π series which bears his name, had a different derivation. It was, in effect, $r \tan^{-1}(x/r) = x - \frac{x^3}{3r^2} + \frac{x^5}{5r^4} - \frac{x^7}{7r^6} \dots$ where x is $r \tan \theta$ for some angle θ in a circle radius r . Hence we deduce, for $\theta = \pi/4$ radians, $\pi/4 = 1 - 1/3 + 1/5 - 1/7 \dots$. To anyone familiar with the production and integration of infinite series it is clear that this series is produced by expanding and integrating

⁷⁶GTU 168.72

$r^2/(r^2+x^2)$ between 0 and x . This was almost certainly James' derivation, and David must have been able to detect this possibility. Thus his investigations were directed towards justifying the choice of $r^2/(r^2+x^2)$ as 'elementum', rather than towards a truly independent derivation of the series.

Rectification of an arc given its tangent was the most obvious approach. Given a circle radius r , and a tangent AC to it of length x , we wish to measure the arc AF .



Let BC be δx , and join DB , cutting the tangent EF to curve in H . Draw HK parallel to BC . We require to sum the infinitesimal elements HF . Let $HF = \delta s$, we must find $\delta s/\delta x$.

Using the facts that $\angle DFH$ is right, triangles DHK and DBC are similar, and so are triangles ADC and FHK , we deduce

$$\frac{\delta s}{\delta x} = \frac{r^2}{r^2+x^2-x\delta x} \longrightarrow \frac{r^2}{r^2+x^2} \text{ as } \delta x \rightarrow 0$$

$\Rightarrow ds/dx = r^2/(r^2+x^2)$ as required. In this analysis, we take DH as $\sqrt{DF^2 + HF^2}$, and do not assume H lies on the circle and $DH = r$.

When Gregory approached the problem, he followed his usual practice of allowing BC or δx to be 1, and so attempted to measure HF in units of BC . Such a procedure, however, is insufficient in this case, where we must let $\delta x \rightarrow 0$ to find the required answer.

In the Exercitatio, however, it seems that he must have followed a procedure similar to that I have suggested above, for he states simply

$$[ds/dx =] L = \frac{(r^2 x \sqrt{r^2 + x^2}) - r^3}{r^2 + x^2 \cdot 3/2 - r^3 - rx^2} \left[= \frac{r^2}{r^2 + x^2} \right].^{77}$$

On expansion and term by term integration the required result appears.

Among Gregory's papers we find two alternative derivations of this result, in each of which he looks for HF corresponding to a unit increment of BC. The first of these papers gives an analysis similar to that outlined above, except that he assumes H is on the curve and so $DH = r^{78}$. With $\delta x = 1$, this gives him

$$HF = \frac{rr}{\sqrt{r^2 + x^2} (\sqrt{r^2 + x^2} - 2x + 1)}$$

If he had kept $BC = \delta x$, he would have found

$$HF = \frac{rr \times \delta x}{\sqrt{r^2 + x^2} \sqrt{r^2 + x^2} + (1 - 2x) \delta x}$$

which, on dividing both sides by δx and then letting $\delta x \rightarrow 0$, gives the required result.

However Gregory's entire statement (and thus conception) of the problem would not allow of such a step. Yet, knowing the required answer, he was well aware that the term $-2x+1$ must be omitted from his expression. He can only justify this by stating that $-2x+1$ will not enter into the reduction to infinite series. He can only mean by this that the expansions of $1/\sqrt{r^2 + x^2}$ and $1/\sqrt{r^2 + x^2 - 2x + 1}$ are the same, which is simply untrue, whether we proceed by mechanical means or by the binomial theorem. The latter expression will always contain odd powers of x , which can never enter the former. Yet Gregory was to make a similar assertion later to Colin Campbell⁷⁹. He cautioned here that we must not disregard $-2x + 1$ in the course of the computation, but only as a final step.

⁷⁷Exercitatio 41

⁷⁸Misc. 6

⁷⁹Gregory to Campbell 25/1/1686 CCC.

His other approach ignored this caution⁸⁰. Throughout, Gregory assumed that although $BC=1$, B and C can, whenever required, be taken as coincident. Yet at the same time he assumed that the ratio of HF to HK , or even the absolute value of HF were valid, well-defined concepts. Thus, he argues

$$\begin{aligned} HE & \stackrel{(\ominus)}{=} \frac{AD \times BH}{AB} \stackrel{(\ominus)}{=} \frac{AD \times FC}{AC} = r \frac{\sqrt{r^2 + x^2} - r}{x} \\ BE & \stackrel{(\ominus)}{=} \frac{BD \times BH}{AB} \stackrel{(\ominus)}{=} \frac{DC \times FC}{AC} = \frac{\sqrt{r^2 + x^2} (\sqrt{r^2 + x^2} - r)}{x}, \end{aligned}$$

Where the equality sign (\ominus) indicates the assumption that B and C are coincident, or an equivalent assumption such as $\hat{BHE} = \pi/2$.

Next we assume DB parallel to DC

'since the points B and C differ only by an infinitely small space; that is, they coincide'.

Hence, $EB : EH = EC : EF = HK : HF$

$$\Rightarrow HF = \frac{HK \times EH}{EB}$$

We find HK from $DB:BC$ "(or 1 since the BC are equal, or infinitely small)" = $DH:HK$, by letting $DB = DC = \sqrt{r^2 + x^2}$ and $DH = DF = r$.

On resolving, this plethora of unwarranted assumptions neatly gives us

$$HF = \frac{r^2 \sqrt{r^2 + x^2} - r^3}{r^2 + x^2 \cdot 3/2 - r^3 - rx^2} = \frac{r^2}{r^2 + x^2}, \text{ as required.}$$

(Gregory's argument here is unnecessarily long: we need only say $HF/HK =$

$EF/EC = AD/DC$, and $HK/DH = BC/DB$, which all follow from strictly

similar triangles, to get $HF = HK \times AD/DC = \frac{BC \times DH}{DB} \times \frac{AD}{DC} =$

$$\frac{BC \times DH}{DB} \times \frac{r}{\sqrt{r^2 + x^2}}. \text{ Assuming now } DH = r, DB = \sqrt{r^2 + x^2} \text{ and } BC = 1,$$

we have $HF = r^2/(r^2 + x^2)$. However, these final assumptions are, without further examination, no more justified than the many similar ones which

litter Gregory's analysis. Following his pattern we could assign many different values at will to HF.

It seems that this approach was Gregory's preferred one. In January 1686 he wrote in reply to Colin Campbell's query about this result,

'My value which as yee observe is $L = v^2/(v^2 + x^2)$ is found by taking AB for AC and AF for AH, since minime different as Dr. Barrow uses to say. And your $L = v^2/(v^2 + x^2 - x)$ will not creat any different series from mine if you divide as they stand. So I choosed the most simple'⁸¹.

(It is interesting to note that Campbell's value for L shows he had followed the path I outlined initially for this analysis, although with $\delta x = 1$. That is, he had deliberately avoided the (unnecessary) assumption $DH = r$).

Gregory's contention that these two expressions give the same infinite series is again simply untrue. It is possible, by 'cheating' over the order in which the remainder is written at each stage of the division, to force out the same answer from each. However, this involves an ever-increasing 'dead weight' of remainder at each stage. Surely both men could see that any such artificial process could only produce a meaningless result. Yet Campbell apparently never challenged Gregory's claim.

This ability of Gregory's to satisfy himself with any sort of invalid justification for a procedure which led to a result of whose validity he was convinced, reappeared frequently in his mathematics.

⁸¹Gregory to Campbell 25/1/1686 CCC.

Here, and typically, his justification rested on an unsupported assumption of equality between infinitesimal quantities. The trait was to become more marked in his Oxford work, where he frequently found himself struggling with new mathematical techniques which were too complex for him. His embarrassments over the catenary curve and Cassini's planetary orbit arose out of just this over-readiness to accept invalid arguments leading to apparently valid conclusions⁸².

⁸²See e.g. catenary curve, chap.4, or Cassini's oval, chap.5.

3.6 Reactions to the Exercitatio

Colin Campbell and John Craige discussed Gregory's first work and the examples and problems found in it. However, it was not their opinions Gregory cared for, but Isaac Newton's. Probably, too, he hoped (as he may have with his submission to the Royal Society on Sluse's method of tangents) to catch the attention of the scientific world in England.

On 9th June, 1684, David wrote to Newton, enclosing a copy of the work and asking for Newton's

'free thoughts and character of this exercitation, which I assure you I will justly value more than that of all the rest of ye world'⁸³.

The letter is humbly phrased, and explains that from the letters of Collins and James Gregorie, David has learnt that Newton, too, has developed similar methods, as he has acknowledged in the Exercitatio. He hopes to know especially how much Wallis intends to publish of Newton's method of resolving an equation into an infinite series.

'which is infinitely troublesome and tedious to me'⁸⁴.

It makes no mention of anything Gregory learnt on his trip to London, but we would hardly expect it to do so.

Newton apparently left this letter unanswered, but it did not pass unnoticed. Intending at first to forestall Gregory's promised sequel, Newton fell to writing another tract on his methods⁸⁵. He was, in Dr. Whiteside's words

⁸³Gregory to Newton 9/6/1684 NCII no. 268 p.396.

⁸⁴ibid.

⁸⁵MPIV 413-419 and preface.

'considerably put out to have priority of publication of a particular case of his binomial theorem and certain allied expansions into infinite series so suddenly snatched away from him and from so unexpected a quarter'⁸⁶.

The tract which he was provoked into writing was the 'Specimina', and his comments on Gregory were clearly designed to depress any pretensions he might have to the invention of these methods. He even insisted that James had derived his results from a study of Newton's example for the zone of a circle. Newton emphasised that his method would

'be described in such a way that Mr.(David) Gregory's book, with its more lavish explanation of points which are here touched upon piecemeal and omission of others which are here more copiously described may profitably fill the role of an introduction'⁸⁷.

However, as he wrote, Newton's intentions altered, and he began to be more concerned with answering Leibniz's criticisms of his method. Finally the treatise became the abstract 'De calculo serierum' which mentioned neither Gregory nor Leibniz and was abandoned in the middle of the third chapter.

Otherwise, the book made little impression. It was reviewed in the Transactions, but this review merely lists its contents and explains Gregory's concept of 'elementum'⁸⁸. The author, it says, assumes the doctrine of indivisibles and the arithmetic of infinites and, by considering these 'elementa', applies these matters to various cases. He expands expressions into infinite series by division or

⁸⁶ ibid XIX.

⁸⁷ ibid 530, in Whiteside's translation 531.

⁸⁸ PT XIV (1684) no 163 730

root extraction. The reviewer does not mention the reduction of equations to infinite series which is clearly used, if not explained, in the book. Indeed, the review devotes as much space to the historical introduction as to the mathematics in the work. It describes James Gregorie's published work and gives Newton's zone series ending with David's hopes that Newton's work will soon be published. The book was certainly not altogether original, but some results and methods here appeared in print for the first time. Some of the results were only otherwise known in James' manuscript letters. While we would not expect wild enthusiasm in the Transactions reviewer, the tone of the review is nevertheless extremely low key.

Dr. Whiteside suggests that this reviewer was John Wallis, whose forthcoming Algebra was to include some of this work, as done by Newton⁸⁹. Gregory's book appeared while Wallis' was printing and was duly acknowledged in it⁹⁰. John Wallis, partly as general defender of English priority in any discovery and more particularly as the first to be allowed to publish anything of Newton's on these methods, cannot have been pleased to see Gregory's work. It did not give the general binomial theorem, nor explain the method of resolving equations into infinite series, which two inventions made up Newton's contribution to the Algebra. However, it gave two particular cases of the former and used the latter in three examples. It would not be surprising to learn that Wallis indeed wrote that review, and deliberately stressed the historical angle and Newton's part in it, rather than describing the new aspects of the work.

⁸⁹D.T. Whiteside 'David Gregory' DSB V 520-2, 522.

⁹⁰Wallis op cit (22) 347.

Of course, the work was not as new and exciting as it might have been. Had James Gregorie lived to write his own account of his methods, developments in the calculus might have gone rather differently. Yet this work, apart from Newton's reaction and the friendly interest of such as Campbell and Craige went largely unnoticed.

Meanwhile, of course, new developments in calculus were taking place. Newton's work appeared in part in Wallis's *Algebra* the following year, and in the same year as Gregory's Exercitatio was published, Leibniz published in the Acta Eruditorum the paper which first set out the fundamental theorem of the calculus⁹¹. With such innovations Gregory's work (with or without the help of a preview of Newton's methods) could not compete. Some thirty years later the Exercitatio was noticed by continental mathematicians, but not in the way Gregory had hoped.

⁹¹Leibniz 'Nova Methodus....' AE Oct., 1684 467-73.

3.7 The π series in the Exercitatio

James Gregorie's series for \tan^{-1} , sent to Collins in 1671, leads directly to the series $\frac{\pi}{4} = 1 - 1/3 + 1/5 - 1/7 \dots$ which converges, although slowly, to give a value for π ⁹². ~~However,~~ Leibniz' ~~published the series~~ explicitly in the Philosophical Transactions for 1682⁹³. When the squabble over priority in calculus blew up in the eighteenth century, the question of priority in this series between Gregorie and Leibniz became one of the issues.

In his Exercitatio, David had given the \tan^{-1} series, implying at least that it was his uncle's. However, he attributed the π series to the 'most famous geometer', Leibniz, deriving it from his uncle's \tan^{-1} series⁹⁴. He also gave the series in his lectures on practical geometry, but without attributing it to any inventor⁹⁵.

Indeed, the whole priority question was a little ridiculous here (if not in the major quarrel as well!). Leibniz had written to Huygens with the series in 1674,⁹⁶ and had certainly derived it independently of James who, as far as we know, did not state his \tan^{-1} series explicitly as a series for π .

Wallis, too, attributed the series to Leibniz in 1685⁹⁷, but, of course, David as the rival inventor's nephew, was the prime witness for the continental mathematicians. Johann Bernoulli implied that the inclusion by the editors of the Commercium Epistolicum of James' \tan^{-1}

⁹²GTV 168-72.

⁹³PT (Philos. Collect.) no.7 April, 1682 204.

⁹⁴Exercitatio 42.

⁹⁵Geometry 97.

⁹⁶NC VI 6 n 5.

⁹⁷Wallis op cit (22) 347.

series in his letter to Collins of February 1671 was no more than forgery⁹⁸. He suggested that it might have been slipped in among the other (presumably genuine) series in that letter simply to discredit Leibniz and steal priority from him. If James had the series, he argued, surely his own nephew knew of it. In which case, why would he allow priority of the π series to Leibniz?

It is clear, of course, that Gregory knew perfectly well that his uncle deduced the series for \tan^{-1} , and that Leibniz's π series can be deduced from it. As a young mathematician, writing his first book, he wished to put nobody's nose out of joint and refrained from underlining his uncle's priority in the general series, while giving Leibniz all credit for the particular case. It was simply unfortunate that this piece of politeness should rebound as it did, so that Gregory's Exercitatio was remembered longest for the one point in it which Gregory (as he saw the quarrel with Leibniz build up) must have most regretted.

⁹⁸Johann Bernoulli to Leibniz 12/5/1714 NCVI no 1075 131-3.

3.8 Gregory's second method; 'that scandalous theft'

In the Epistola Posterior, Newton outlined his 'abrupt series', but it was not published until 1693⁹⁹. Meanwhile, David Gregory had, in 1688, published the same series, derived in a different manner¹⁰⁰. John Craige claimed at the time, and, more vociferously, thirty years later that Gregory had merely reconstructed the series from what Craige had told him of Newton's work¹⁰¹. Samuel Horsley was prompted by this to suggest that the relevant papers should be published, to expose 'that scandalous theft of Dr. Gregory's'¹⁰². The remainder of this chapter is mainly concerned with the justice of Craige's claims.

The series in question, as sent to Leibniz in 1676, effected the quadrature of the curve $L = dz^{\sigma} \times (e + fz^{\eta})^{\lambda}$. Putting $\sigma+1/\eta = r$, $\lambda + r = s$, $d/\eta f \times (e + fz^{\eta})^{\lambda+1} = Q$ and $r\eta - \eta = \pi$, the area under this curve is

$$Q \times \left\{ \frac{z^{\pi}}{s} - \frac{r-1}{s-1} \times \frac{eA}{fz^{\eta}} + \frac{r-2}{s-2} \times \frac{eB}{fz^{\eta}} - \frac{r-3}{s-3} \times \frac{eC}{fz^{\eta}} + \text{etc.} \right.$$

where A,B,C ... denote the preceding term. When r is a positive integer, this series breaks off after r terms. Newton did not say so, but made it clear by example that we may rewrite the equation as $L = dz^{\sigma + \eta\lambda} \times (f + ez^{-\eta})^{\lambda}$, in which case the series will break off if $\frac{\sigma + \eta\lambda + 1}{-\eta}$ is a positive integer. In each of these cases then, the 'abrupt' series gives a finite expression for the area under a curve.

Professor Turnbull suggested that Newton derived the formula by integration by parts. If we put $L = z^{\pi} z^{\eta-1} (e + fz^{\eta})^{\lambda}$, where

⁹⁹ John Wallis Opera II (Oxford, 1693) 390-1.

¹⁰⁰ in the Solutio.

¹⁰¹ John Craige De calculo fluentium ... (London, 1718) Preface.

¹⁰² Quoted MP VII 21n2. 'On an obsolete cover now loose in ULC Add. 4005' written on Oct. 23, 1777.

$\pi = \sigma - \eta + 1$ as above, we have

$$d \int z^{\pi} z^{\eta-1} (e + fz^{\eta})^{\lambda} dz = \frac{dz^{\pi} (e+fz^{\eta})^{\lambda+1}}{(\lambda+1)f\eta} - d \int \frac{\pi z^{\pi-1} (e + fz^{\eta})^{\lambda+1}}{(\lambda+1)f\eta} dz$$

Rewriting $e + fz^{\eta}$ as u and considering $u^{\lambda+1} = u^{\lambda} e + u^{\lambda} fz^{\eta}$ in the final term, we have

$$d \int z^{\sigma} u^{\lambda} dz = \frac{dz^{\pi} u^{\lambda+1}}{(\lambda+1)f\eta} - \frac{d\pi e}{(\lambda+1)f\eta} \int z^{\sigma-\eta} u^{\lambda} dz - \frac{d\pi}{\eta(\lambda+1)} \int z^{\sigma} u^{\lambda} dz .$$

$$\Rightarrow d(1 + \frac{\pi}{\eta(\lambda+1)}) \int z^{\sigma} u^{\lambda} dz = \frac{z^{\pi} Q}{\lambda+1} - \frac{d}{\lambda+1} \frac{e}{f} \frac{\pi}{\eta} \int z^{\sigma-\eta} u^{\lambda} dz .$$

Multiplying by $\lambda + 1$

$$d(\lambda + r) \int z^{\sigma} u^{\lambda} dz = z^{\pi} Q - d(r-1)e/f \int z^{\sigma-\eta} u^{\lambda} dz .$$

Repeated applications of this formula give Newton's result.

Pitcairne's Solutio of 1688 was designed primarily to establish Harvey's priority in discovering the circulation of the blood. It set up rules for determining first discoverers, and, as an example of this gave a series discovered by David Gregory. However, apart from Gregory's inclusion of a constant of integration to supply the definite integral between 0 and x , instead of Newton's indefinite one, this series which Pitcairne published was Newton's 'abrupt' series.

The method of discovery, however, was somewhat different. Gregory started by assuming that $\int (bx^r \times (sx^n + a)^m) dx = (sx^n + a)^{m+1} \times \text{U.F.}$ where U.F., or the 'universal factor' is some series. He then expanded $bx^r \times (sx^n + a)^m$ into an infinite series and integrated it term by term. He divided the resulting series by the infinite series expansion of $(sx^n + a)^{m+1}$, and the resulting series was his universal factor. The method is somewhat messy and, with no convergence consideration, not very rigorous, but it works. The universal factor is Newton's 'abrupt' series.

In 1718, John Craige explained that he had spent some time in Cambridge in 1685. Newton had been friendly to him and had shown him his 'abrupt' series. Craige continues

'Returning later to my own country I fell into friendship with the famous Dr. Pitcairne and Mr. D. Gregory: to whom I showed what sort of series Mr. Newton had for quadratures, which both confessed was completely unknown to them. But some months later Dr. Pitcairne told me that Mr. Gregory had found a series which broke off similarly'¹⁰³.

Newton had given Craige two examples of this series, to help in the arguments he was then composing against Tschirnhaus's methods¹⁰⁴.

These were the quadratures of $y = \frac{x}{m} \times \sqrt{x^2 + a^2}$ and $y = x/\sqrt{m} \times \sqrt{x + a}$, and Craige had passed them on to his fellow Scots along with the information that they were examples of series which break off. It had been easy for Gregory to deduce his series from this information.

On seeing Gregory's publication in the Solutio. Craige had written to Newton asking for a copy of the 'abrupt' series. Newton sent this on 19th September, 1688, and Craige found that it was almost identical to Gregory's. The series (which many, such as George Cheyne, then attributed to Gregory¹⁰⁵) should be attributed to Newton alone.

Among Gregory's papers, and even in the published versions of his 'second method', as he called this series, there is much to support Craige's charges. However, against this there is Gregory's own claim that he had the method in 1684, and used it in the Exercitatio. We shall examine this first.

¹⁰³Craige op cit (101)

¹⁰⁴op cit (65)

¹⁰⁵George Cheyne Fluxionum methodus inversa; sine quantitatum fluentium leges generationes (London, 1703) 6.

3.9 The Exercitatio example

In 1691, Gregory sent his version of the series to Newton, hoping for support in having it published. Probably he knew of Craige's remarks and hoped to forestall them, for he included an extra example which was not in the Solutio.

To rectify the semi-cubical parabola, we must find the area under the curve $L = \sqrt{(1 + 9x/4c)}$. Here $\frac{r+1}{n} = 0$, and we can apply the formula, to find that the area is

$$\begin{aligned} & \left(x + \frac{4c}{9}\right)^{3/2} \times \frac{\frac{3}{2} c^{-\frac{1}{2}}}{\frac{1}{2} + 1} - \frac{\frac{3}{2} c^{-\frac{1}{2}}}{\frac{1}{2} + 1} \left(\frac{4}{9} c\right)^{3/2} \\ &= c^{-\frac{1}{2}}(4/9c + x)^{3/2} - 8/27 c. \end{aligned}$$

Gregory told Newton

'and even then I used this method for the rectification of the curve given on p.13 of the Exercitatio geometrica, since it does not yield to the method of squarings given there'¹⁰⁶.

This result certainly is stated there, but with no explanation of how it was deduced.

From his notes we see that David was at first baffled by this integral. However, on 23rd August, 1682, he could give the following rule;

if $y = (ab + ax)^{\frac{1}{2}}$, then the area under the curve is $\sqrt{\frac{4}{9}} a \times \overline{b + x^{\frac{1}{2}}} - \sqrt{4/9} ab^3$ or, $\frac{2}{3} a^{\frac{1}{2}} \times \overline{b + x^{3/2}} - \frac{2}{3} b \times (ab)^{\frac{1}{2}}$. He also noted that he now wished a similar result for $y^2 = ab + a/x$!¹⁰⁷ This rule allowed

¹⁰⁶Gregory to Newton 7/11/1691 NCIII 374 172-6; in Turnbull's translation 176-9, 177.

¹⁰⁷C 196.2.

him to rectify the semi-cubical parabola and to measure the surface of a parabolic conoid. However, this note gives no details on how the formula was derived.

The semi-cubical parabola, $x^3 = cy^2$, had been one of the first curves rectified. In the late 1650's, von Heuraet and William Neile had both done so, and Brouncker had improved Neile's result¹⁰⁸. The rectification which Gregory gave can be easily deduced, for example, from the treatment given by Wallis of Neile's rectification, and this was published with his De Motu, with which we know David was familiar¹⁰⁹. So Gregory's problem was not precisely one of finding $\int (ab + ax)^{\frac{1}{2}} dx$, but of producing an integral which embraced $\int_0^x (1 + 9t/4c)^{\frac{1}{2}} dt = c^{-\frac{1}{2}} (\frac{4}{9} c + x)^{3/2} - 8/27 c$, and probably other known results as well. The problem was not rendered trivial thereby, but was certainly made easier.

Now, when he considered the areas represented by the indefinite integrals of various parabolae, Gregory had come on the case $y^2 = p(a + x)$. Here he clearly knew $\int_0^x p^{\frac{1}{2}}(a+t)^{\frac{1}{2}} dt = 2/3 p^{\frac{1}{2}}(a + x)^{3/2} - 2/3 p^{\frac{1}{2}} a^{3/2}$ (although not, of course, in this notation). The indefinite integral gave him only the first of these terms, which did not represent the area he required. He noted that this curve

'is not operated according to the above canon [that of the Exercitatio] but $\dots \overline{x + a^{\frac{1}{2}}}$ is treated as if it were x , so it is no wonder that the required area does not arise, but that which has $\overline{a + x^{\frac{1}{2}}}$ for x' .¹¹⁰

¹⁰⁹Wallis Opera 1 (Oxford, 1695) 551.4.

¹¹⁰C196.24

This most unclear statement may be interpreted in several ways, but it seems to indicate a first step towards the justification he would later give Campbell for his rule.

There was another aspect of his attack on this problem which had more in common with his later 'second method'. In his notes on rectifying the semi-cubical parabola, Gregory cited the rule for $(ab + ax)^{\frac{1}{2}}$ and added

'the truth of this is clearer if it is examined by infinite series'¹¹¹.

That is, by expanding $(ab + ax)^{\frac{1}{2}}$ and integrating term by term we reach the same series as if we expanded $\frac{2}{3} a^{\frac{1}{2}} (b + x)^{3/2} - \frac{2}{3} a^{\frac{1}{2}} b^{3/2}$.

This suggests a method by which Gregory might have derived the formula. By inspection of known examples and analogy with the rule for single powers of x , it would be easy to deduce that

$$\int_0^x (ab + at)^{\frac{1}{2}} dt = \frac{2}{3} a^{\frac{1}{2}} (b + x)^{3/2} - A,$$

where A is some constant. Expansion of both sides in infinite series with integration of the left side, leads at once to $A = \frac{2}{3} a^{\frac{1}{2}} b^{3/2}$.

It is important to notice here, though, that there is no division of series by series, nor any hint (here or elsewhere before the letter to Newton in 1691) that this answer represents a broken off series.

Gregory was to employ this same technique first in solving individual cases, and later in finding constants of integration in the general case, for his 'second method', but there is no similarity to his derivation of the general result there.

¹¹¹C196.17

Both Colin Campbell and John Craige were puzzled by finding this result in the Exercitatio. In January, 1685, Gregory sent Campbell a copy of the work, and two months later he wrote back to explain this point.

'Sit $L = \sqrt{ab + ax}$, then $L = a^{\frac{1}{2}} \times \overline{b + x}^{\frac{1}{2}}$ and because x is not alone (to be treated according to the lemma) but joined under a radicall sign with b , I treat both together as if they were the x , that is $\frac{2}{3} a^{\frac{1}{2}} \times \overline{b+x}^{3/2}$, and from that I take b so treated because it ought to have been so used and then you have the area = $\frac{2}{3} a^{\frac{1}{2}} \times \overline{b + x}^{3/2} - \frac{2}{3} a^{\frac{1}{2}} \times b^{3/2}$, 112.

These comments are strongly reminiscent of his remarks on the curve $y^2 = p(x + a)$, and the implication is that he discovered the rule by inspection and a happy intuition. If he used infinite series to derive (and not only to check) the result, he did not care to confess this to Campbell. In March, 1685, this sort of verbal justification seemed more compelling to him than the use of series as I have described above.

John Craige expressed his doubts of the result to Campbell, who replied with Gregory's justification. Craige wrote back protesting¹¹³. Certainly, he said, the rule holds in the case $L = b \times a^{-\frac{1}{2}} \times \overline{a + x}^{\frac{1}{2}}$, over which he had first queried Campbell, but suppose $L = (ax - ab)^{\frac{1}{2}}$? Here (since the curve is undefined for $x < b$) the area is simply $\frac{2}{3} a^{\frac{1}{2}} \times (x-b)^{3/2}$ and we need not compensate for treating $x-b$ as if it were x .

¹¹²Gregory to Campbell 14/1 & 5/3 /1685 CCC.

¹¹³Craige to Campbell 9/9/1687 CCC.

Thus he neatly punctures the justification for Gregory's rule, by considering it (the talk of compensating for treating $x + b$ as x) as a general rule, of which $(ax + ab)^{\frac{1}{2}}$ is merely a special case. In applying such a rule he points out

'you must always be chopping and changing accordingly as new cases happen'.

The rule of the Exercitatio, he warns Campbell, should be restricted to cases where y is expressed in simple powers of x , that is, where $y = \sum_r a_r x^r$. As he says,

'It is no hard matter to make rules of this kind comprehend cases for which they were never intended, providing that we know the Quaesitum some other way'.

Of course, all Craige says is perfectly justified, and it would have been interesting if he had put these points to Gregory. Perhaps he might then have been stimulated to examine the basis of his rule more closely and have reached some truly independent discoveries. Craige was not, however, being entirely open with Campbell here. In 1686 (the year before he wrote this letter) Craige had published a paper in the Transactions which explained the use of limits of integration, as Newton had taught him¹¹⁴. Its application to this problem should have been obvious to him, but he kept silent about it.

Thus, Gregory certainly did not discover his rule for the quadrature of $(ab + ax)^{\frac{1}{2}}$ as a special case of a more general rule. Nor did he find it in the form of a breaking off series. However, it is an example of his 'second method', so perhaps he developed this method from this first example?. This seems highly unlikely.

¹¹⁴P.T. XVI no.183 (July-Sept.,1686) 186-9.

Only two months after he had written to Campbell about this expression, Gregory wrote again to tell him that

'I have latelie deduced from [the quadrature methods in the Exercitatio] ane other which is infinitely more gentile'¹¹⁵.

Yet even when he sent Campbell two examples of this other method (the 'second method', as subsequent letters make clear), he still did not mention the earlier rule in this context. Nor does this example play a part in his early calculations of the 'second method'.

Gregory's comments to Newton in 1691, quoted at the start of this section, were not outright lies. It is true that the rectification of the semi-cubical parabola is an example of the 'second method', and that Gregory published it in the Exercitatio. It does show that in 1684, before he knew John Craige, Gregory was thinking along broadly these lines and had probably devised the method he would later use to solve individual cases of the rule and find suitable constants of integration for his 'universal factors',

However, this example was not part of a general method and Gregory could not in 1685 give any satisfactory justification for it. It is not until Gregory mentions 'ane other method' to Campbell in May, 1685,¹¹⁶ that we can suppose he then had some idea of his 'second method'.

¹¹⁵Gregory to Campbell 2/10/1686 CCC

¹¹⁶ibid

3.10 Evidence for Craige's claim

To evaluate Craige's claim we must answer several questions. First, did Gregory have his 'second method' before he met Craige? As we saw above, he seems only to have had the method in May, 1685, but unfortunately we know only that he met Craige in that year, and not whether it was before or after that date. This question must be left undetermined for the moment.

Secondly, did Craige tell Gregory and Pitcairne all he claimed? That is, did he give them these two examples and tell them of the general nature of the series? Thirdly, could Gregory then 'easily' deduce his result? Finally, why did John Craige nurse his bitterness for thirty years before attacking Gregory in print? These last two questions are answered in subsequent sections. Here I will show that it is most likely that Craige told Gregory and Pitcairne all he claimed, and that the use Gregory made of the knowledge makes it highly unlikely that he knew of the method beforehand.

Firstly, in spite of the letter to Campbell in May, 1685, Gregory was in 1686 still establishing the theorem. The first examples of the method were sent to Campbell in October, 1686 and by then he was already considering publishing¹¹⁷, but of all the papers on this method (most of them dated) none bears a date earlier than 1686. Indeed, all of these are concerned with individual cases; only in 1687 did the general theorem appear.

One of the papers implies that Gregory knew from the start the origin of 'his' method. On a paper dated 1686 he wrote down the

¹¹⁷Gregory to Campbell 2/10/1686 CCC.

expressions

$$L = dx^\sigma \times (m + x^\eta)^\lambda$$

$$L = dx^\sigma \times (m + nx^p + x^\eta)^\lambda$$

$$L = dx^\sigma \times (m + sx^q + nx^p + x^\eta)^\lambda$$

and notes that this is the series of curves for which we wish to find canons analogous to 'the Newtonian canon' for the first of them.

Interestingly, the notation here is closer to Newton's than to Gregory's final form.

Below this he multiplied out the forms for $\lambda = 1, 2 \dots$ and integrated term by term. Thus, for example, with $\lambda = 2$ in the expansion of $L = dz^\sigma \times (e + fz^\eta)^\lambda$ he derives the area

$$\frac{d e^2 z^{\sigma+1}}{\sigma+1} + \frac{2d e f z^{\sigma+\eta+1}}{\sigma+\eta+1} + \frac{d f^2 z^{\sigma+2\eta+1}}{\sigma+2\eta+1}$$

He then noted the 'multiplicatores' , $z/(\sigma+1)$, $z/(\sigma+\eta+1)$, $z/(\sigma+2\eta+1)$, corresponding to each of these three terms respectively. That is, he noted the factors by which each term of the expansion of $dz^\sigma(e + f z^\eta)^2$ must be multiplied to give this area. This paper shows then, not only his awareness of a Newtonian canon's existence, but also that he associated it with a series multiplying a binomial factor - although here he has taken the wrong factor¹¹⁸.

This seems compelling evidence that Gregory had learnt of Newton's series, of the expressions for which it was valid and something of its form. It also suggests that his comments to Campbell in May, 1685 were exaggerated more than a little! Or perhaps, of course, the remarks referred to some other method, abandoned swiftly when he heard of this one.

¹¹⁸C206.

There were also the examples Newton had given Craige. These were

$$1. \int x/m (x^2 + a^2)^{\frac{1}{2}} dx = 1/3m (x^2 + a^2)^{3/2}$$

$$2. \int x/\sqrt{m} (x + a)^{\frac{1}{2}} dx = 2/5\sqrt{m} (a + x)^{3/2} (x - 2/3a)$$

When Gregory sent three examples of his method to Campbell in October 1686, two of them were these two¹¹⁹. The first, certainly, was in Craige's Methodus, published in 1685¹²⁰ and Gregory might have known of it from that source. It was mentioned several times in the papers as Gregory tried to reconcile his definite with Craige's indefinite integration¹²¹, but the second curve is also mentioned briefly in the same context at least once¹²². Certainly by October 1686 Gregory knew both these curves and, significantly, they were two of the first three he thought of when sending examples to Campbell.

However, the most conclusive evidence, both that Gregory knew of Newton's work before he started studying the matter, and that Newton's work was in fact the most influential condition on Gregory's work, was simply the form of his proof. Throughout the years 1687-8, and perhaps even earlier, Gregory derived his universal factor in special and general cases. The calculation is repeated again and again among his papers. Yet on each occasion he begins with something of the form $\int bx^r \times (sx^n + a)^m dx = (sx^n + a)^{m+1} \times U.F.$ Nowhere is there a hint at an alternative derivation.

Yet, on the face of it, this is a meaningless assumption. Why should we choose a universal factor which fits this particular format? Why choose a universal factor at all? Without prior knowledge, we

¹¹⁹Gregory to Campbell 2/10/1686 CCC

¹²⁰Craige op cit (104) 42

¹²¹e.g. C204

¹²²C205

would expect simply to exchange one infinite series expressing the integration of a binomial for another one. Clearly, such a step would only be made by someone who (from Craige's hints and a study of his examples, say) realized that this particular formulation was likely to give a finite factor.

Newton's derivation, through integration by parts, is totally different. It is the application of a powerful method to a particular case where it brought useful results. Gregory's derivation shows at once that he did not fall upon his series through purely independent research. Probably this was why Newton had ignored any hints Craige had dropped when Gregory first published, but was furious when he saw the paper with his method of derivation.

This same argument also strongly implies that Craige was also right in suggesting that Gregory and Pitcairne knew nothing of the method before they met him. Had they known it, they must have had an independent derivation, and Gregory need not have produced this one.

In sum, then, Craige's claims seem to be true. He gave Gregory the two examples of Newton's method, and told him that they were examples of a series which broke off under certain conditions. In the next section we will examine the use Gregory made of this information.

3.11 Development of the method

From the time he heard from John Craige of Newton's method until after his publication of it in 1688, Gregory continued to develop and examine the series. He made several further discoveries about it, but his major difficulty was adapting the basic series, an indefinite integral, to the definite integral defined between 0 and x . Craige's paper in the Transactions in 1686 which supplemented his Methodus gave a brief but clear account of how to discover this definite integral¹²³. Yet Gregory either had not read this, or did not realise its application to his problem. It is significant that Craige, although he explained the point somewhat to Colin Campbell¹²⁴, apparently never explained it to Gregory.

In 1686, Gregory continued to examine the problem, but seems to have concluded little more in the general case than that the quadrature of $bx^r \times (sx^n + a)^m$, m a positive integer, was finite¹²⁵. However, he had evaluated several individual examples, notably those he sent to Campbell, which he may have done by a process of expanding out infinite series, much as he had earlier solved the case $(ab + ax)^{\frac{1}{2}}$. The fact that he was here finding the definite integral (which this method gives) bears this out.

As an example of this, consider $L = x \times (x^2 + b)^{\frac{1}{2}}$, which is basically Craige's first curve, and suppose

$$\int x(x^2 + b)^{\frac{1}{2}} dx = A \times (x^2 + b)^{3/2} + B.$$

Then we have on expanding

¹²³op cit (114)

¹²⁴Craige to Campbell 1/9/1687 CCC

¹²⁵C206

$$\begin{aligned}
 \text{L.S.} &= \int \{ b^{\frac{1}{2}} x + x^{3/2} b^{\frac{1}{2}} - x^{5/2} b^{3/2} + x^{7/2} b^{5/2} - 5x^{9/2} b^{7/2} \dots \\
 &= \frac{1}{2} b^{\frac{1}{2}} x^2 + \frac{1}{8} b^{-\frac{1}{2}} x^4 - \frac{1}{148} b^{-3/2} x^6 + \frac{1}{128} b^{-5/2} x^8 - \frac{1}{256} b^{-7/2} x^{10} \dots
 \end{aligned}$$

$$\begin{aligned}
 \text{R.S.} &= A \times \{ b^{3/2} + 3/2 b^{\frac{1}{2}} x^2 + 3/8 b^{-\frac{1}{2}} x^4 - 1/16 b^{-3/2} x^6 \\
 &\quad + 3/128 b^{-5/2} x^8 - 3/256 b^{-7/2} x^{10} \dots \} + B
 \end{aligned}$$

Examination gives us $A = 1/3$ and $B = -b^{3/2}/3$

$$\text{Thus } \int x (x^2 + b)^{\frac{1}{2}} dx = 1/3 (x^2 + b)^{3/2} - b^{3/2} / 3 .$$

The constant, $-b^{3/2}/3$ is a simple constant of integration which makes our integration correspond to the definite integration between 0 and x . This follows because our integrand is expanded in such a way as to give a power series $\sum_{r=1}^{\infty} a_r x^r$ on integration. Since such a series is zero when $x = 0$, the indefinite integral so formed is automatically the definite integral, as John Craige would have understood. Since the finite expression of the area is derived from this one, it too, gives the definite integral.

To Gregory, this expansion, as he had given in the Exercitatio, was 'correct'. Other answers were wrong. Yet John Craige had given only the indefinite integral for this and other similar problems, and David worried about the difference. He concentrated especially on this curve, $L = y/p \times (m^2 + y^2)^{\frac{1}{2}}$, and tried to resolve the discrepancy into a simple matter of finding which answer was correct. To do so, he used two results of Barrow's which, geometrically expressed, are the analytical equivalents of

$$R \times g(x) = \int f(x) dx \Rightarrow R \times g'(x) = f(x)$$

$$\text{and } g(x) = \sqrt{(2 \int f(x) dx)} \Rightarrow f(x) = g(x) \times g'(x) \quad 126 .$$

Putting Craige's curve as $f(x)$, and his own and Craige's integrations

¹²⁶Isaac Barrow Lectiones geometricae (London, 1669) Problem VII p.125.

of it in turn for $\int f(x)dx$, he tried to reach a contradiction. Of course, since the two integrations differed only by a constant he could not do so. The curves $L^2 = b+x$ and $u = b^2 x/(a^4 + b^2 x^2)^{\frac{1}{2}}$ gave rise to the same apparent anomaly¹²⁷. (This last curve, called 'Craig's second curve' by Gregory, was the third example he sent to Campbell with the two we know Newton gave Craig. Perhaps Newton had given this one also as an example of his method).

Gregory never resolved this to his satisfaction, and could only conclude weakly that we can never say an ordinate corresponds to any one area. He continued to study other curves through the year, and in at least one case he found the constant of integration by performing the calculation I have outlined for $x(x^2 + b)^{\frac{1}{2}}$ above. His example was 'Craig's second curve', $L = x/p \times (m^2 + x^2)^{\frac{1}{2}}$, and he clearly knew the indefinite integration of the curve before beginning this analysis¹²⁸.

Gregory did discover, by his examination of this and other curves, that if we expand the brackets with x first in them, we do find the indefinite integral. In these cases the series expressing the area is not finite, and there is no constant term and hence no constant of integration corresponding to evaluation of the series at $x = 0$. From alternative expansion of $L = (b + x)^m$ [as $b^m(1 + x/b)^m$ or as $x^m(1 + b/x)^m$], Gregory concluded that the true area is found when b is put first [that is, from expanding $b^m(1 + x/b)^m$],

'whence it seems the same may happen in more intricate cases, indeed that value is to be preferred in which b is put first'¹²⁹.

Thus he found some partially satisfactory explanation of his and

¹²⁷C204

¹²⁸C205

¹²⁹ibid

Craige's different results, which both fitted into Barrow's formulae, which he had used as checks. Still, he had no understanding of the true nature of the problem.

However, it seems that in 1686 he was still handling only individual cases. It was in 1687 that he learnt how to cope with the general case $L = bx^r \times (sx^n + a)^m$. In that year, or perhaps at the end of 1686, he first performed his typical long division of one series by another, to find the universal factor for the curve $bx^r \times (sx^n + a)^m$.¹³⁰ The division is an awesome procedure, but presents no basic difficulties. By this stage, too, Gregory was handling the binomial theorem with confidence, perhaps after discussing it with Craige. He now recognised that the universal factor was finite when $\frac{r+1}{n}$ was a positive integer, and he rewrote it for $\frac{r+1}{n} = 1, 2 \dots$ or a general c .¹³¹

At this stage, though, he lost the constant of integration which had appeared in the individual examples. It was some time later that this constant, or 'quantitas ablatitia seu addita' as Gregory called it, reappeared in his work.

In July, 1687 (probably with Pitcairne) Gregory sent several curves to Craige who was then back in Cambridge, challenging him to find their quadrature¹³². These were all of the form $bx^r \times (sx^n + a)^m$, where $\frac{r+1}{n}$ is a positive integer, generally 1 or 2. Gregory looked at these curves and their expansions individually, and again concluded that the constant of integration was lost when the expansion was carried

¹³⁰C208

¹³¹C206

¹³²C205

out with x placed first inside the bracket. He was led eventually to reexamine his universal factor with this problem in mind.

In February, 1687, Gregory had written to Campbell that his method was still unpublished because he had been too busy. He added,

'Besides, I would gladly see Newton's methods which I am

certainly informed will be published in that *Astronomie*'¹³³

The Principia did not contain this method, but it held much else to interest Gregory. From September, 1687 to April 1688 he was busy with the Notae, his commentary on the work. It was only at the end of May, 1688, that he turned again to his 'second method' probably in anticipation of its publication in September in Pitcairne's Solutio.

On May 31st he again calculated the universal factor in the general case, deriving it as before by a division of series. This time he noted also that the series representing the area under $bx^r \times (sx^n + a)^m$ need only be continued to the $m+1^{\text{th}}$ term when m is a positive integer. In early June he examined several particular examples of this case, and persuaded himself thus of its general truth, although he seems not to have proven it for the general case. When proceeding thus, we need only multiply the first term of the expansion of $(sx^n + a)^{m+1}$ by all $m+1$ terms of the series, the next by the first m , and so on until the constant term multiplies nothing and is omitted¹³⁴.

However, the question of the 'quantitas ablatitia ~~ser~~ addita' was soon brought again to his attention in the case $L = x^5 \times (x^2 + a)^2$. Here both $r+1/n (= 3)$ and m are positive integers and so the series contains only three terms. Examining by series in Gregory's manner,

¹³³Gregory to Campbell 2/2/1687 CCC.

¹³⁴C206

we have $x^5 \times (x^2 + a)^2 = x^9 + 2x^7a + x^5a^2$, so that the area = $1/10 x^{10} + 1/4 x^8a + 1/6 x^6a^2$. This corresponds to the Exercitatio method and is the definite integral from 0 to x . But, applying the 'second method' and deriving the universal factor from the formula we have area = $(x^2 + a)^3 \{x^4/10 - ax^2/20 + a^2/60\} = \frac{1}{10} x^{10} + 1/4 ax^8 + 1/6 a^2x^6 + a^5/60$, an indefinite integral, with the extra $a^5/60$.

Gregory's first reaction was that $a^5/60$ was to be neglected, since, as it had been put as the second term in the bracket it should be assumed small, making $a^5/60$ negligible! For, of course, his work so far had suggested that when x was put first in the bracket there was no constant of integration. However he was not altogether satisfied with his initial explanation, and wondered even then if $a^5/60$ might not be this mysterious 'quantitas ablatitia seu addita'¹³⁵.

Accordingly, on June 8th he reexamined his conclusions and found the universal factor again, this time in ascending powers of x . He could now see by examination that to produce the same results as he had found in the Exercitatio he must subtract a^{m+1} X the last term in the series¹³⁶.

Of course, in the most general case, with an infinite series, this instruction is meaningless, but the apparent anomaly lies only in Gregory's way of expressing the process. His first method always (except where $\int 1/x dx$ was involved) gave him a polynomial $\sum_{r=1}^{\infty} a_r x^r$ with no constant term. This then equalled the definite integral from 0 to x . Similarly, in the second method the general infinite case contained no constant term and so had given him an apparently

¹³⁵C208

¹³⁶C197

satisfactory result. However, when $\frac{r+1}{n}$ is a positive integer, say, the term whose index is $r-cn+1$ is a constant and this term, multiplied by a^{m+1} will be the value of the series when $x=0$. Thus Gregory's instructions are precisely equivalent to those generally given today for finding the definite integral. Yet still he did not understand why this happened. He had no conceptual basis for his statement, but was only concerned with effecting agreement between his first and second methods.

This result, however, enabled Gregory to return to the example which had puzzled him, $L = x^5 \times (x^2 + a)^2$. The 'quantitas ablatitia seu addita' turned out to be $a^5/60$ and his two methods now agreed over this integration¹³⁷.

Gregory's researches were at this stage when he wrote the piece for Pitcairne's Solutio which was dated 1 September, 1688. He had calculated the universal factor, such that $\int (bx^r \times (sx^n + a)^m) dx = (sx^n + a)^{m+1} \times \text{U.F.}$, and determined the constant of integration which gave agreement with his first method (and so made the indefinite integral into the definite one from 0 to x). He knew the series became finite when $\frac{r+1}{n}$ was a positive integer, and had satisfied himself that, if m was such, only $m+1$ of its terms need be considered. He summarised these points in a paper written on September 3rd. and added, as he did in the Solutio, that similar rules could be found for the multinomial by anyone who was not afraid of the arithmetic involved¹³⁸.

However, he did not finish studying the series with its publication, although he swiftly came across two apparently intractable problems.

¹³⁷C208
¹³⁸C206.5

In August Craige had given him a paper containing the finite quadrature of $y^2 x^7 = a^8 x - a^9$ ¹³⁹. Gregory wrote this as $y = a^4 x^{-7/2} (x-a)^{1/2}$, where $\frac{r+1}{n} = -5/2$, and he did not see how Craige's result was possible. In September he expanded the series into an infinite series and integrated, but the resulting series was irreducibly infinite and seemed to have no connection with Craige's expression for the area ¹⁴⁰.

On the same day he examined the application of his rule to $y = dx^e \times (sx^n + a)^m$ where $a = 0$. This reduces to the area beneath $y = ds x^{e+nm}$, or $\frac{ds}{e + nm + 1} x^{e + nm + 1}$ ¹⁴¹. However, at around this time he also tried the case where $s = 0$, which should give $\frac{da^m}{e+1} x^{e+1}$, but s enters the denominator of all terms in the series which then become infinite ¹⁴². Gregory was never able to resolve this problem.

It was in October on a visit to St. Andrews, where his brother was then a regent, that David solved the problem posed by Craige's example. Perhaps the general solution arose out of a reexamination of this case, for he now saw that since $bx^r \times (a + sx^n)^m = bx^{r+nm} \times (ax^{-n} + s)^m$, those curves also have a finite quadrature where $\frac{r+1+nm}{-n}$ is a positive integer ¹⁴³. On November 5th he returned to his original notes on Craige's example which he was now able to solve ¹⁴⁴. Finally David had reconstructed all Newton had sent to Leibniz about this series in 1676.

¹³⁹C211

¹⁴⁰C211 and C205

¹⁴¹C208

¹⁴²C200

¹⁴³C208

¹⁴⁴C211

Gregory always felt he had a right to claim some share in this series. How else would he have dared send it in 1691 to Newton himself to ask for an opinion? From his point of view, he had stumbled across a method of integrating $(ab + ax)^{\frac{1}{2}}$ while writing his Exercitatio and, using this method, he had reproduced the examples Craige had shown him of Newton's method along with some other similar ones. He then, by a cumbrous division process, was able to devise at last a general formula for the procedure. Through much confusion and study he had solved, to his own satisfaction, the problem of the 'quantitas ablatitia seu addita'.

Yet without those first examples he would probably not have gone on with this study. Without knowing from Craige that these were examples of a broken off infinite series he would never have looked for such a series. Certainly the help he had from Newton's work was vital, yet Gregory could not, as Craige suggested, 'easily' reproduce the series from there. His own efforts must have long overlain in his mind any help he had gained from Newton's work, and in his opinion, the method was always his - if not by right of first discovery, at least as co-inventor. The final irony lies in his long struggle with the constant of integration. Newton, or Craige, would have seen and solved his problem at once. With any understanding of the concept of limits of integration there is no problem, but for Gregory, discovering this constant had meant several years of study, computing and comparing infinite series.

We cannot measure the amount of Gregory's indebtedness to Newton in developing this series, but we can see why he felt he had some claim to it as his own.

3.12 John Craige's work and his relationship with Gregory

In the 1680's, while he developed his methods of quadrature, Gregory was also studying the work of John Craige. The impulse which led Craige to publish his indictment of Gregory in 1718 must have had its roots in these early days and a study of the two men's reactions to each others' work will help us to see how his resentment may have arisen. In many ways, indeed, Craige was a better mathematician, although Gregory was by no means totally outclassed by him. John Craige's work also helps to give us a better understanding of Gregory's abilities: it is all too easy to compare Gregory only with Newton, Leibniz, James Gregorie, the Bernoullis or other great men of that period. In doing so we must find serious faults in his mathematics. However, in comparing his work with Craige's, a highly gifted mathematician although not a man of genius, we can form a fairer estimate of David Gregory.

Craige was interested at this time in developing Barrow's geometric theorems in an analytic fashion. His Methodus, published in 1685, was based on one of these, which is analytically equivalent to $\int (y \times dy/dx) dx = \frac{y^2}{2}$.¹⁴⁵

In a paper which internal evidence dates as 1687 or later, Gregory examined this book and its use of Barrow's theorem¹⁴⁶. If we wish thus to integrate $z = g(x)$, we must find $y = f(x)$ where the subtangent of $f(x)$ $(y \times dy/dx) = g(x)$. The required integral then equals $\frac{y^2}{2}$. Craige had applied this to many examples, including several of those in Gregory's Exercitatio. David seemed unexcited by the method (which,

¹⁴⁵Craige op cit (65). Barrow op cit (126) Prob. XIX p.22.

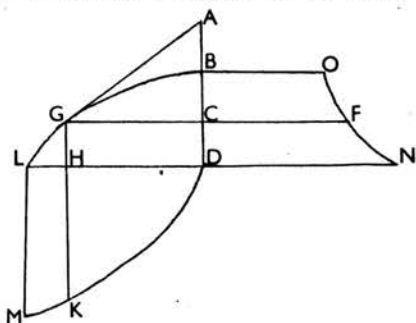
¹⁴⁶C200. This paper refers to examples sent to Craige at Cambridge and so is to be dated 1687 or later.

CR, 17's dated 1686.

especially in the inverse of Leibniz's method of finding a tangent, involved a lot of tedious algebra, but was nevertheless a very interesting development) and was content mainly to criticize the results. He emphasises his own prior publication of these and also the fact that he generally found a result (rectification, zone, area etc. for an ellipse and then indicated the changes of sign or constants necessary to give the equivalent result for an hyperbola or circle. Craige had investigated each of these cases separately. Gregory made no attempt to analyze the basis of Craige's method here, seeming content with its agreement in specific cases with his results in his Exercitatio.

He did agree with Craige's structures on Tschirnhaus' method, but doubted whether the counter example, $y^2 p^2 = (m^2 + x^2)x^2$, could be integrated by Craige's method. This was the first example Newton had given Craige, and here if we needed it is further evidence suggesting that Craige told Gregory that he had been given the quadrature of this curve by Newton. Otherwise Gregory must have assumed Craige had integrated it by his own method.

But the Methodus contained another of Barrow's theorems - the change of variable theorem which Craige was to develop in later work. In August 1688, he gave Gregory a paper on this theorem, which Barrow had himself regarded as one of his most fertile¹⁴⁷.



Stated geometrically, the theorem is thus; given any curves BGL and DKM, referred to perpendicular axes AD and DL, with a third curve OFN such that, for any point G of BGL,

¹⁴⁷A95. Barrow op cit (126) Theorem IV Lecture XII p.129.

with GF parallel to DL, cutting AD at C, GK parallel to AD cutting DL at H and DKM at K, we have $AC:CG::HK:CF$. Then area DHK = area BCFO and area DLM = area BDNO.

Analytically, we can restate this so, with AD the x-axis, DL the z-axis, DN the y-axis, LM the v-axis. Let DKM be $v = f(z)$; BGL, $z = g(x)$; OFN, $y = h(x)$. We have constructed OFN such that $\forall x$, $AC:CG::HK:CF$, that is, $1:g'(x) = f(z):h(x)$. Therefore,

$$\begin{aligned} h(x) &= f(z) \times g'(z) = f(z) \times dz/dx \\ \text{Now } DHK &= \int_0^Z f(z) dz = \int_0^Z f(z) \times dz/dx \times dx \\ &= \int_0^x h(x) dx, \text{ where } Z = g(X) \\ &= OBCF. \end{aligned}$$

Similarly, $DLM = BDNO$.

That is, by changing the variable in which our first curve, $f(z)$, was expressed, we have equated its integral to that of another curve.

We hope, of course, that the second will be easier to integrate than the first!

This example indicates superbly the scope of Barrow's geometry, and the generality of the results which he could achieve. Gregory's work on infinite series was only one aspect of a broad onslaught on problems of quadrature and Craige's developments of Barrow's work showed quite another approach.

However, Craige's handling of the theorem in the Methodus was not above criticism, and Gregory was able to detect the fault¹⁴⁸. Craige had earlier determined the quadrature of $z = r^{2/3} y^{1/3}$ as $3/4 r^{2/3} y^{4/3}$. Now he attempted to do the same with Barrow's theorem. We may

¹⁴⁸Craige op cit (65) problems 4 and 19; C201.

paraphrase his argument in modern notation as follows:

given $r^2 v = x^3$ whose quadrature we wish to determine, change the variable by putting $v^2 = ry$, giving us $\int^3 \sqrt{r^2 v} dv = \int^3 \sqrt{r^2 r^{\frac{1}{2}} y^{\frac{1}{2}}} . \frac{1}{2} r^{-\frac{1}{2}} y^{-\frac{1}{2}} dy = \frac{1}{2} r^{\frac{4}{3}} \int y^{-1/3} dy = \frac{3}{4} r^{\frac{4}{3}} y^{\frac{2}{3}} = \frac{3}{4} r^{\frac{2}{3}} v^{\frac{4}{3}}$ as before.

But Craige did not take this last step and left his answer as $\frac{3}{4} r^{\frac{4}{3}} y^{\frac{2}{3}}$, different from $\frac{3}{4} r^{\frac{2}{3}} y^{\frac{4}{3}}$ as found above. He argued from this that an infinite number of quadratures can thus be found for this, and so for any, curve. David Gregory correctly identified the error, and stated definitely that we cannot have two different areas corresponding to the same ordinate.

The situation is much less clear in the Methodus, where it is couched in geometrical terms. Craige's error was less obvious, and less amazing, than it seems when written in modern notation. Consequently, it was less easy for Gregory to detect. This example shows that David was adept in this form of calculus too, and not only in his infinite series methods. As Craige could, and did, make valid criticisms of his work, so Gregory could also criticize Craige's.

The paper Craige gave Gregory on this matter explained the use of the theorem in practice¹⁴⁹. Clearly, given BGL and DKM it is easy to find OFN (provided we can find the tangent to BGL), but it is more difficult, and far more useful, to find BGL given DKM and OFN. That is, given an integrand, and a differentiable function expressing a change of variable, it is easy to find a new integrand. It is more difficult to find a suitable change of variable which will reduce a

¹⁴⁹A95. August 1688.

given integrand to another given one. Craige had devised a method for tackling this latter problem by assuming a multinomial with undetermined coefficients for the change of variable function, BGL. Evaluation of these coefficients involves much long and tedious algebraic manipulation. He did solve many curves in this way, of the type Gregory was also then interested in, such as $ay = (x^3 + ax^2)^{\frac{1}{2}}$ and $ya^2 = 3x^2 \times (x^3 + a^3)^{\frac{1}{2}}$. A more complicated example was $4a^3y = a^{\frac{1}{2}}x^{\frac{1}{2}}(x^3 + 8x^2 + 5ax + a^2)^{\frac{1}{2}}$ which Gregory's methods could not solve. It was this paper which integrated $yx^{7/2} = (a^8x - a^9)^{\frac{1}{2}}$, which puzzled Gregory. As a general case, he considered also the curve $y = x^e \times (x^m + a)^{1/s}$

Gregory had two other papers on this method of Craige's. One deals specifically with the general case $L = ax^m \times (bx^n + d)^e$ ¹⁵⁰. Another considers particular examples such as the transmutation of $y^3 = ax^2$ into $v^2 = az$ ¹⁵¹. Craige also sent Campbell an additional explication of the algebra involved in his method, and Gregory had made a copy of it¹⁵².

In September, 1688, Gregory examined his results in the context of Craige's method¹⁵³. As Gregory writes here, we can use Craige's canon to derive $\int n/m x^{n/m-1} (a + x^{n/m})^{r/s} dx = \int (a + z)^{r/s} dz$, by putting $x^n = z^m$, and this leads to the first case of Gregory's rule, when $r+1 = n$. He wished changes of variable could be found for the general case $r+1 = cn$, c a positive integer.

¹⁵⁰C199

¹⁵¹A96

¹⁵²Craige to Campbell 19/12/1687 ; C202.

¹⁵³C211, 14 September, 1688.

Certainly Gregory had a great interest in Craige's method, but he apparently made no attempt to examine or enlarge its basis. His main concern was with its agreement or otherwise with his own method and with the results it produced. It appears that it was not the method itself which interested him so much as its inferiority or superiority to his own. This may be unfair; Gregory may have written other papers, now lost to us, in which he examined the method for its own sake. However, on the basis of the papers we still have, there is no reason to suppose he did so.

We can gauge Craige's opinions of Gregory's work only from two letters written to Colin Campbell¹⁵⁴. On September 1st, 1687, he wrote of the different values he and Gregory had found for the quadrature of $z^2 a^2 = a^2 y^2 + y^4$. Here, as usual, Gregory had subtracted a constant term to find the definite integral from 0 to y, while Craige gave the indefinite integral. This letter explained their inconsistencies in terms of the geometric model Craige used in the Methodus of $\int y \times dy/dx \, dx = 1/2 y^2$. He did not touch explicitly on limits of integration, but clearly explained that the two values simply corresponded to different base lines. The letter he wrote eight days later was that criticizing Gregory's justification of his integration of $(ab + ax)^{1/2}$. His comments on that occasion were unanswerable.

In both these letters, Craige wrote of matters which puzzled Gregory and which, with Newton's help, he could explain. Yet Gregory continued to be puzzled, and wasted many hours puzzling over problems arising out of the difference between definite and indefinite integration.

¹⁵⁴Craige to Campbell 1/9/1687 and 9/9/1687 CCC.

Craige appears never to have passed on his knowledge.

When the two men first met in 1685, they had every reason for friendship. Gregory had an established academic position at Edinburgh, and Craige had all the reflected glory of one lately returned from visiting Newton. Craige was apparently very open with Gregory and Pitcairne. He told them what he knew of Newton's 'abrupt' series and the 'tractatus de seribus infinitis' which is in Gregory's hand among his papers probably represents the notes he made at this time on the extracts Craige had made from Newton's 1671 tract on fluxions¹⁵⁵.

Yet at some stage a definite note of rivalry entered their relationship. In the summer of 1687, Gregory was sending curves to Cambridge, challenging Craige to integrate them¹⁵⁶. He realised that Craige had not reconstructed Newton's theorem from the hints he had passed on. They criticized each other's work; Gregory in his private papers and Craige in his letters to Campbell. Perhaps they also voiced these criticisms to each other. Gregory kept his method a secret until its publication, and Craige never gave the help he could have given over limits of integration.

When Craige finally saw the Solutio two factors must have stirred his resentment. First, he had betrayed Newton's trust by telling the methods he had learnt in confidence to another who had published them. Secondly, the method of which Gregory boasted, and by which he solved the problems he had challenged Craige to solve, was not his own at all.

¹⁵⁵A56. MP III 354-72. Dr. Whiteside's opinion on the provenance of this paper (as I have given it above) 354-5 nl.

¹⁵⁶C205.

He wrote to Newton asking for a copy of his 'abrupt' series method, and less than three weeks after the publication of the Solutio Newton replied with the extract from his Epistola Posterior. Craige now saw his suspicions confirmed.

It was in Campbell's eyes that Craige first determined to vindicate himself by showing up Gregory's fraud. In January, 1689, he wrote to him, sending the extract from the Epistola Posterior. He added

'& I must tell you be the by that I saw this series at Cambridge and acquainted Dr. Pit[cairne] and Mr. Greg[ory] with it, and told them the chiefe propertie of it, sc: that it breaks of when the figur's Quadrable, at which time they were altogether ignorant of such a series as I can let you see by the letters of the Dr: written to me at Cambridge; which astonished me to find no mention made of Mr. Newton by the dr. but keep this to your selfe'¹⁵⁷.

Campbell apparently ignored the charge, which is not mentioned again in what we have of his correspondence.

Perhaps Craige also wrote to Newton at this time, for three years later, in 1691, Newton had certainly heard Craige's side of the affair¹⁵⁸.

Craige had a moderately successful career in the church, being collated by Bishop Burnet to the prebend of Dunford in 1708 which he later exchanged for the prebend of Gillingham Major. However, his work on calculus never achieved for him the recognition he desired. His work on change of variable, for example, was an interesting new approach, equal to the work Gregory was putting out at the time.

¹⁵⁷Craige to Campbell 30/1/1689 CCC. Partly in NC III 325, 8-9.

¹⁵⁸Nov, 1691 NC III 376, 181-2.

Unfortunately, though, its application was limited. Today we use this method generally by substituting with trigonometric functions, a concept which was out of Craige's reach. J.F.Scott describes him as 'unusually gifted' and remarks that he deserves remembrance for his mathematics¹⁵⁹. Yet, although he was made a Fellow of the Royal Society in 1711, his talent was (and still is) largely unrecognized. Meanwhile, David Gregory, who had been his equal in the 1680's, consolidated his position as Oxford's Savilian Professor of Astronomy, Newton's trusted friend and a leading Newtonian scientist. And this was the man Craige believed he had caught out in a piece of flagrant dishonesty.

We know almost nothing of their relationship after Gregory had gone to Oxford. They exchanged letters about the 'second method' in 1691, when Gregory hoped to publish it and apparently hoped to be able to disprove Craige's claims of plagiarism¹⁶⁰. One letter which passed between them has survived, written by Craige to Gregory on 11th April 1695. This is friendly in tone, asking for news of developments in the mathematical world and discussing the work of various continental scientists. It is largely taken up with a suggestion for a paper showing that all the papers in the Acta Eruditorum have been taken from earlier authors. We may read sarcasm in Craige's remark

'I have an extraordinary desire to know what

improvements you have made of Mr. Newton's philosophy'¹⁶¹, but perhaps none was intended!

On October 1st, 1703, Pitcairne warned Colin Campbell to

¹⁵⁹J.F.Scott 'John Craige' DSB III 458. See also DNB IV 1373.

¹⁶⁰Craige *op cit* (101) preface

¹⁶¹Craige to Gregory 11/4/1695 Bod. MS Tanner 24 fo.20.

'take note that Mr. Craig is very far from being
a friend to Dr. Gregory'¹⁶²,

but he gives no details of their disagreement. He described himself as 'great with all' so we may assume that he was still in touch with Craige.

There were probably two motives for Craige's publication in 1718 of his charges against Gregory. First there was his long rankling bitterness against the man who had betrayed Craige's trust by publishing as his own matters passed on in confidence, but had nevertheless achieved so easily the recognition which passed Craige by. Secondly, it may have been a last bid for Newton's recognition, which by that time meant the recognition of the British scientific world. In his preface to the De Calculo Fluentium Craige was recalling to Newton's mind the days when he had been one of the first privileged to see Newton's private work. He was also accusing someone of plagiarizing from Newton - a popular charge at that time! Thus the preface was designed both to settle a thirty year old score and to bring himself into the limelight.

In the event, it did neither. Craige continued to live in obscurity, and Gregory's reputation was almost undamaged. Certainly the charges left some stain on Gregory's character, but few have accepted Craige's charges as easily as Horsley did¹⁶³. As the charges were unsubstantiated, he had apparently been given the benefit of the doubt. Modern authors quote Craige's remarks, but few give a verdict¹⁶⁴. Now we can see from Gregory's papers that Craige's

¹⁶²Pitcairne to Campbell 1/10/1703 CCC.

¹⁶³See n102.

¹⁶⁴Turnbull quotes the charges but does not comment on Gregory's guilt or innocence NCIII 9n5. Whiteside is clear on Gregory's guilt MPVII 3-10.

charges were true in substance but misleading in implication. He had indeed told Gregory and Pitcairne what he claimed and it had been crucial to the development of Gregory's 'second method'. Yet it had not been an easy step to develop the method from these hints, and Gregory worked for several years before he achieved all Newton had for his series.

3.13 Publication with Newton and Wallis's compromise

When Gregory arrived in England in the summer of 1691, hoping to win his way to the Savilian chair of astronomy, Newton received him well and wrote him a glowing reference for the post. Clearly he was not remembering any tales Craige had told him of the source of Gregory's 'second method'. Eventually, though, this method was raised between them.

'Mr. Hally and others' advised Gregory that he should publish an account of the method in the Transactions - probably to prove himself in the eyes of the electors to the Savilian chair¹⁶⁵. Consequently Gregory asked leave to send Newton a copy of the method and

'since, by what you have told me, I know that ye have such a series long agoe I entreat ye'll tell me so much of the historie of it as ye think fitt I should know and publish in this paper'¹⁶⁶.

On November 7, 1691, Gregory sent his method to Newton, much as it appeared in the Solutio¹⁶⁷. He also included the case where $\frac{r + mn + 1}{-n}$ is a positive integer, which he had only discovered after that publication. Here, too, he inserted the case $(1 + \frac{9x}{4C})^{\frac{1}{2}}$ from the Exercitatio implying that he had the method then. Newton was not convinced by this evidence, and was extremely angry.

Now Gregory's first letter shows that he and Newton had discussed the method amicably, and Newton cannot then have regarded it as taken from his own work. Either he heard from Craige only after this date, or the sight of the series itself persuaded him.

¹⁶⁵Gregory to Newton 10/10/1691 NCI III 372, 169-70

¹⁶⁶ibid

¹⁶⁷Gregory to Newton 7/11/1691 NCI III 373, 170-2, with enclosure NC III 374 172-6, my quotations from Turnbull's translation 176-9.

On October 10th, the day he wrote to Newton asking permission to send him a copy of the method, Gregory wrote also to John Craige. The latter's Dé Calculo Fluentium tells us of this letter which said Gregory intended to lay the tale of the development of his method before the world. Craige commented darkly that it would have been better had he done it then rather than leave the tale for himself to give¹⁶⁸.

Indeed, had he received Newton's cooperation in telling the tale as he saw it at this time, it would certainly have been better for Gregory to have told it then. Yet he can hardly have intended the full-scale confession of his theft which Craige implies. It is highly unlikely that Craige had not long ago pointed out to Gregory the identity of his method with Newton's but Gregory clearly had no qualms about sending Newton his own version. To him, the work he had put into the series, especially in determining the constant of integration, made it his. Gregory's conscience was untroubled. Yet even had he suffered from some pangs of conscience this would not have been the moment to confess. His chances of the Savilian chair hung on the good opinion of the English scientific world, and he was not the man to throw that away for the sake of a troublesome conscience. If he intended anything of the sort Craige implied he would have waited until he was secure in the Savilian chair.

However, Craige might not only have replied to Gregory's letter on the matter. It may be that this was the stimulus which persuaded him to write to Newton, whose draft letter to Gregory about the series says that he was not aware of the trouble over it when it was first

¹⁶⁸Craige op cit (101) preface.

begun¹⁶⁹. This would explain Newton's early amicability and later anger on the matter.

Alternatively, it may be that he had heard and dismissed Craige's charges until he saw Gregory's derivation of the series. For here, as we saw, is a method which proclaims Gregory's foreknowledge that he would achieve a worthwhile answer through an apparently artificial step. The derivation of Gregory's method shows that it was most unlikely to have been an independent discovery. This, coupled with a memory of charges Craige had made some years earlier, may have convinced Newton.

Although he had earlier agreed to publish a paper of his own with Gregory's¹⁷⁰, his reaction to this 'second method', for whatever reason, was very different. His draft reply, which may never have been sent, evidently took some labour to compose, for it is a mess of alterations and cancellations. It is, however, quite firm in stating that the series is Newton's and in refusing to ignore the part played by Craige in transmitting information to Gregory. Newton said -

'But your fellow countryman Craige also, when he stayed with us for quite a long time six years ago, examined my manuscripts (as he himself declares in his book published at the time). It was then that he sent to you my squaring of that curve. When a discussion about it arose, conducted by correspondence, you attacked the squaring of curves afresh and hit upon your series. You know, however, that on Craige's subsequent return to Scotland he confirmed that he

¹⁶⁹ Draft Newton to Gregory ? November, 1691? NCIII 376, 181-2, quotations from Turnbull's translation 182-3.

¹⁷⁰n 167 p.171.

had seen my series. This was when I myself had not heard that a dispute about the matter had been stirred up by him, now that you had discovered the series; nor had you, I think, been informed that I had previously hit on a similar series¹⁷¹.

Newton, while maintaining perfect civility, and giving Gregory the benefit of every doubt, had no intention of overlooking the impetus his own work had given the Scot. He would not help Gregory to refute Craige's allegations by supporting his publication of the series. The proposed article in the Transactions was forgotten.

However, Newton was stimulated by receiving the 'second method' into writing his first tract 'De Quadratura Curvarum'. Dr. Whiteside describes his response;

'the pattern of his private reaction to this new stimulus from Gregory to review his earlier researches into the quadrature of curves almost exactly parallels that of his earlier response in June 1684 to Gregory's transmission of a copy of his Exercitatio Geometrica [see section 6]: once provoked his interest in the matter passed almost immediately from specifying and clarifying the historical context of their discovery to refining and reshaping their verbal exposition and then to developing their content and enlarging their application'¹⁷².

Thus in the revised 'De Quadratura' which Dr. Whiteside dates in early

¹⁷¹n 169. 183.

¹⁷²MP VII 10-11.

winter 1691/2, Newton merely mentioned that John Craige had written to his fellow Scots the quadrature of $a^4z/(c^4 - 2c^2z^2 + z^4)$. Thereafter, David Gregory had fallen on the same series 'by a different, though indeed not inelegant, method'¹⁷³.

The correspondence between Newton and Gregory which had grown up over the summer was halted. Three weeks after he had sent his series, David wrote Newton a brief note asking if it had come to him and begging for his opinion¹⁷⁴. Newton may have replied to this, but thereafter their correspondence ceased.

Gregory and Newton met again in December, 1691, after the appointment to the Savilian chair had been made. Newton was cordial, and advised Gregory on his inaugural speech and told him of his plans to publish a book of geometry¹⁷⁵. Thereafter the two did not meet again for $2\frac{1}{2}$ years.

It does not seem that Newton showed Gregory any open anger when they met in December, but no invitations to Cambridge were issued until the spring of 1694. The affair of the 'abrupt' series must have given Newton a fairer idea of Gregory's mathematical abilities, and have left him less well disposed than formerly. It is possible, too, that the return of Fatio de Duillier, the favourite disciple, in early September, 1691, left Newton less inclined to encourage Gregory.

In the meantime, though, Newton was not allowed to forget Gregory and his 'second method'. In the 1685 edition of his Algebra, John Wallis had printed excerpts from Newton's letters to Leibniz,

¹⁷³MP VII 50.

¹⁷⁴Gregory to Newton Nov. 26, 1691 NCIII 375, 181.

¹⁷⁵C85 RG fo. 70,71 NCIII 381 191.

but not the abrupt series. Now Gregory became his colleague at Oxford, and he determined to publish the Scot's method in the 1693 edition of this work. Probably this was mainly to help the younger man establish himself in the scientific world, but it may also have been a deliberate spur to Newton to allow the publication of his own method.

Gregory gave his method to Wallis in a letter dated 21st July, 1692, which mentioned the prior publication in the *Solutio*. Gregory related the method again much as he had to Newton, but with slightly different examples. Pitcairne published it and

'He then believed that this series was known only to me, but a little later he discovered that the same series had been found by that great philosopher and geometer Mr. Isaac Newton, who had arrived at it earlier, as I believe, by a different method. It is indeed of concern to the republic of letters that this method is concealed no longer but rather that in an early statement, the Newtonian method should be known no less than ours has already been'¹⁷⁶.

Wallis wrote to Newton, requesting his version of the method, and this time he consented to publication with Gregory. On 27th August and 17th September, 1692, he replied with a generalization of the formula, taken from the newly composed '*De Quadratura*'. Gregory's method thus became only a special case of Newton's general theorem.

¹⁷⁶Wallis *op cit* (99) Gregory's series 377-80, Newton's series 390-1. Quotations in my translation 380.

Wallis's account here called Newton's 1676 series and Gregory's merely 'similar'. He said that the information about Newton's series had reached Scotland after Pitcairne's publication, and did not mention Craige's earlier knowledge of it. The account gives honour on both sides: Newton was the first inventor of a result independently developed later by David Gregory, since which time Newton had enlarged and generalized it. Apparently Newton was satisfied with this - certainly John Craige was not.

Wallis's account seems to have been generally accepted, at least until Craige's charges appeared in 1718. Although Newton would not help Gregory to establish a claim to the series, John Wallis had now done so.

3.14 Other reactions

Gregory's publication in the Solutio was little known, and that in Wallis overshadowed by Newton's. Few showed any reaction to the series, but through Gregory's visit to Huygens it became known to him and to de l'Hôpital.

In 1693, Gregory travelled to Holland where he met Christian Huygens. Pitcairne, then in the chair of medicine at Leyden, had already discussed Gregory's quadratures with the Dutchman, who was keen to know more. During the visit, Gregory continued these discussions and outlined the steps by which he achieved his series¹⁷⁷. On August 12, 1693, after his return to Oxford, he wrote of it to Huygens¹⁷⁸.

Huygens had challenged him with two curves taken from Huighens Observationes¹⁷⁹. One, $y = a^2 x^3 (x^4 - a^4)^{-1}$ has $\frac{r+1}{n}$ a positive integer, and in the other, $y = 4a^4 x (x^4 - a^4)^{-1}$, neither $\frac{r+1}{n}$ or $\frac{r+mn+1}{-n}$ is a positive integer. Nevertheless, Huygens claimed, the first is not quadrable, while the second is. In fact, $\int a^2 x^3 (x^4 - a^4)^{-1} dx = a^2/2 \log (x^4 - a^4)$ and Gregory correctly identified it as a logarithm if we allow this interpretation of the notation he used in the Exercitatio. He wrote the area as $(a^4 - x^4)^0 x^0 a^2/0$ and described it as 'not unlike that contained between the hyperbola and its asymptote'.

The second curve baffled him though. In fact, Huygens' mathematical skills were failing as he grew older and struggled to

¹⁷⁷Huygens Oeuvres Complètes de Christiaan Huygens publiées par la Société Hollandaise des Sciences 22 vols (La Haye, 1888-1950). X 2812 471-3.

¹⁷⁸Gregory to Huygens 12/8/1693 ^{NC} ~~NC~~ III 418, 275-6.

¹⁷⁹Hubertus Huighens Paucae quaedam observationes circa proportionem quam ad rectilineam habent figurae curvilineae breviter tradita (Mittelpburg, 1692) See NCIII 278 n.4.

assimilate all the new methods which were appearing. His claim that the curve was quadrable had arisen from a fault in his calculation¹⁸⁰. Gregory simply promised a further answer when he had had more time to think of it. Almost as an afterthought, he sent Newton's method too.

De l'Hôpital showed an interest in Gregory's series, and Huygens sent him a copy of it¹⁸¹. However, his version did not mention the 'quantitas ablatitia seu addita' which converted the indefinite to the definite integral. Huygens criticized it for giving only the 'quadratura curtata' or indefinite integral. De l'Hôpital was able to point out that we must subtract the indefinite integral evaluated at $x = 0$ to obtain the definite one. It is ironic that these two should criticize Gregory for the very point he had criticized in Craige. Indeed, it is strange that, having taken so much time and trouble to learn how to find the constant of integration, Gregory did not send it with the version he gave Huygens. Eventually, the latter received Wallis's Algebra, and must have seen that Gregory gave the definite integral there, but he did not mention this in his letters.

Again, though, Gregory's work had made little impression. Pitcairne, Campbell, John Wallis, Huygens, de l'Hôpital and so presumably many others accepted it as Gregory's work and accorded it a greater or lesser degree of admiration. But by the time of John Craige's charges in 1718, David Gregory's discovery, independent or otherwise, of the first case of Newton's general theorem cannot have been of much importance. Perhaps its most important result for Gregory was the contribution it may have made to the $2\frac{1}{2}$ years of silence between himself and Newton. Otherwise it was forgotten by all but John Craige.

¹⁸⁰Huygens op cit (177) x 463 n 19.

¹⁸¹Huygens to de l'Hôpital 3/9/169 3 ibid 2819 esp. 412-3.

Chapter 4

The New Calculus

Whether or not Gregory felt snubbed by Newton after their meeting in December, 1691, which preceded the $2\frac{1}{2}$ year break in their relationship, he lost none of his enthusiasm for Newton's work. Once he had settled into his new life at Oxford, in December, 1692, Gregory resumed his Notae on the Principia, and took every opportunity to discuss Newton's work with Fatio de Duillier.

In early summer, 1693, Gregory travelled to Holland and met Huygens. On his return, influenced perhaps by the Dutchman, or perhaps by John Wallis, he began work on his first paper for the Transactions, which was published in January, 1694. By this date, too, his Notae were complete. At last, in May, 1694, perhaps through the influence of Fatio, Gregory was reconciled with Newton.

The visit he then made to Cambridge proved one of the most important points of his life. Gregory discussed many aspects of Newton's work with him and thereafter became one of his trusted confidants. From this point on, Newton's influence would become more and more important in Gregory's work.

The first result of this new influence was the 'Tract on fluxions' where Gregory expounded Newton's methods of fluxions and fluents. This was written in autumn, 1694 and probably intended for Gregory's students at Oxford. Besides Newton's work, it contained many examples from the Acta.

In 1695, other concerns occupied Gregory, but his delight in Newton's fluxions led him to resume their study in early 1696. Then he began his workbook E, with the specific intention of studying the

work of the continental mathematicians, published in the Acta, and he continued this study until at least the middle of 1697. Generally, he recast the work of continentals in fluxional notation, provided any proofs they had omitted and added a few minor corollaries of his own. He had intended to add these to his 'Tract on fluxions', but this intention was never carried out.

In the winter of 1696-97 his researches led him to the brachistochrone and the catenary. In both of these (the first a challenge set by James Bernoulli and the second a curve whose properties the continental mathematicians had published without proofs) he hoped to give public evidence of his own abilities and to show the power of Newton's methods. Unfortunately he erred totally in the first problem, which he was only able to solve after knowing the result and reading the proof of it published by Sault. He did not understand Newton's attempt to explain the problem to him. He published his solution of the catenary curve, however, in a paper which was perfectly competent mathematically, but erred badly in deriving the initial equation from mechanical considerations. Both Leibniz and James Bernoulli criticised this work.

Thereafter, he did little further work on the calculus. In 1697 and 1698 he helped John Wallis collect the letters which Newton and Leibniz had exchanged in the 1670's. He was convinced of Newton's priority in the calculus and at least extremely suspicious that Leibniz might have plagiarized.

From 1697, though, Gregory was more and more taken up with his work on the Astronomiae. He then, following Newton's interests and urged by the University, worked on editions of Euclid and Apollonius.

His final years were also busy with government work. In January, 1704 he found time to plan a major work on calculus; historically presented, it would display Newton's method of fluxions, illustrated by many examples. However, he never found time to develop this beyond an outline draft. Possibly, too, Newton dissuaded him from the project.

In this work we find Gregory's strengths and weaknesses as a mathematician. He very rarely produced an original answer and was on occasion satisfied with inadequate proofs. Newton's mathematics was at times simply too complex for his comprehension. Nevertheless, at his best he produced competent work in a new and difficult field, and was able to explain the work of others in this field with clarity.

4.1 The years of punishment

In November, 1721, William Stukeley stood for election as secretary to the Royal Society against Newton's favoured candidate. On this account he tells us.

'Sir Isaac show'd a coolness toward me for two or three years, but as I did not alter in my carriage and respect toward him, after that, he began to be friendly to me again'¹.

In just the same way, it seems, Gregory was punished by Newton with 2½ years of coolness after the latter had realized just what Gregory had published about the abrupt series. From December, 1691 to June, 1694, Newton and Gregory did not meet. The Notae on the Principia were resumed, but it was only through Fatio de Duillier that Gregory could find help with this work, or news of Newton's latest researches.

However, Gregory was far from being cut off from contact with the scientific community because he had incurred Newton's displeasure. His fellow Savilian Professor was John Wallis, whose work Gregory had long admired. He had used Wallis' Arithmetica Infinitorum for his earliest work in mathematics, and his mechanics lectures were largely based on Wallis' Mechanica². Gregory was later to describe these two works in a speech made at Oxford on Wallis' behalf. Of the former he said

¹ William Stukeley Memoirs of Sir Isaac Newton's Life (London, 1736) 17.

² See, for example, C196; For the mechanics lectures, Chapter 2.8.

'to this work are owed all the advances in Geometry made since then or yet to be made'.

Of the latter he remarked that, in spite of the work of Galileo, Torricelli, Guldini and others, before this work appeared

'there was still no-one who would reduce [mechanics] to the form of a science, who would build it up from its basics and establish definitions, axioms and postulates, and thence deduce the primary and fundamental propositions; which [Wallis], who alone was capable of it, most happily did'³.

The close association at Oxford with a man of whom he had such a high opinion must have gone far to compensate Gregory for Newton's neglect. Indeed, Wallis and Gregory appear to have maintained a most cordial relationship until the former's death in October, 1703.

Also, possibly through family connections, through friends made on his previous trip abroad or through Archibald Pitcairne or Fatio de Duillier, Gregory at last met Christian Huygens in the summer of 1693. The Scot was in Holland in May and June, and he met Huygens on at least three occasions, on which they discussed recent scientific developments⁴. Of course, Newton's work was mentioned, and Huygens' doubts about the doctrine of absolute motion were raised. Nor did the Dutchman accept Newton's theories on the propagation of light. However, he warned Gregory that Newton must not be diverted into the

³ Oxford, 10.7.1703. Misc. 33, 34.

⁴ Gregory's notes of these meetings; A4, A8, A14, A31.

fields of theology and chemistry. He was also keen that Flamsteed should declare publicly for the finite speed of light in the hope that this would persuade Cassini.

The continental scientists, especially de l'Hôpital and the Bernoullis were mentioned in these conversations. If from no other source, Gregory would have known of their work through Fatio, but it is quite likely that Huygens' admiration for their results kindled up his enthusiasm. Gregory also met Hudde on this visit, as well as Pitcairne's medical friends, but it seems from his notes that Huygens made the deepest impression.

It is hard to discern any personal influence (as distinct from the influence of their published works) which Wallis and Huygens had on Gregory. Some individual features of his work, such as the inclusion of de l'Hôpital's rectification of the logarithmic curve in the 'Tract on fluxions', may be traced to this visit to Huygens, but such points are relatively unimportant. All we can say is that it seems probable that Huygens encouraged his interest in the continental work. On returning to England, Gregory sent Huygens his own and Newton's method of abrupt series, to which Huygens replied in January, 1694. However, these letters do not suggest any particular influence which Huygens may have exerted⁵.

Wallis' influence is even less easy to find, although as we have seen, his published work had been a very strong influence on Gregory's early work. The two were, of course, closely connected for many years in University work and in other projects. They collaborated, for

⁵ See Chapter 3.14; Huygens to Gregory: 19.1.1694. Bod. MS Tanner 25 fos 290-91.

example, on the report on ^{mathematics} ~~maths~~ teaching in Christ's Hospital, made in 1694, and in 1697 and 1698 on the collection of the Newton-Leibniz correspondence. Wallis' influence on Gregory's private life may have been stronger, but it is not marked in his Oxford scientific work. However, if Huygens encouraged Gregory's interest in the continental mathematicians, Wallis would certainly have encouraged his interest in Newton's work.

One or both of these men, however, probably lay behind the choice of subject for David's first paper in the Philosophical Transactions. At the end of August, 1692, Wallis received, through a Mr Bridgman, a challenge to solve the Florentine enigma. The problem was couched in terms of Grecian temples and the skill of Ancient geometers: mathematically it was the problem of drawing in a hemispherical dome four equal, similar and similarly placed 'windows', so that, when these were removed, the remaining surface of the dome could be exactly measured.

This problem admits an infinity of solutions, and Leibniz and Bernoulli had both written on it in the Leipzig Acta⁶. Wallis' contribution had been to relate one possible mode of solution to squaring the lunula of Hippocrates, and this was published in the Philosophical Transactions⁷.

Meanwhile, however, Viviani had published a work which gave, without proof, another solution to the problem. Huygens received the book in October, 1692, and wrote to de l'Hôpital telling him of this new solution. He worked on the problem himself, and was able to show

⁶ Leibniz in AE (June, 1692) 275-79.

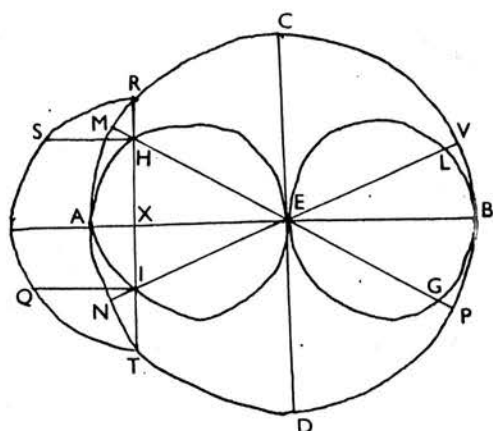
Bernoulli in AE (August, 1692) 370-71.

⁷ PT XVII (January, 1693) no 196, 584-92.

the identity of Viviani's solution with Bernoulli's⁸.

Thus both Wallis and Huygens were concerned with this problem. As Gregory's first attempt to prove Viviani's solution was made on 2nd August, 1693⁹, on his return from Holland, it seems likely that Huygens had first introduced him to the problem, or at least to this solution of it. However, he was unable to solve it then, and Wallis may have encouraged him to tackle it again, on which second attempt he was able to produce a solution for the Transactions.

Viviani's solution was thus; if ACBD is a cross-section of a sphere, pierced by two cylinders, cross-section AHEI, BLEG, then either hemisphere, ACB say, will be pierced by four holes as required in the problem. Its remaining surface will equal AB^2 .



On his first attempt at the problem, Gregory deduced quickly that

the cylinder would cut the sphere at a height AH above any point H. Thus, the remaining surface would be made up of arcs of radius RX, and sine AH. This follows from a consideration of the auxiliary semi-circle RSQT, imagined to stand on RT, perpendicular to the plane of the paper. It follows easily that $AH = HS$, and after the cylinder has been cut out we are left with the arc RS on the surface

⁸ Vincenzo Viviani Formazione ..., (Firenze, 1692).
Huygens to de l'Hôpital: 22.10.1692 in Huygens Oeuvres Complètes de Christiaan Huygens publiées par la société Hollandaise des Sciences 22 vols (La Haye, 1888-1950) X 329.
Huygens manuscript 27.10.1692 ibid X 329.

⁹ C100.

of the hemisphere.

If we let $AX = x$ and $AE = a$, $AH = \sqrt{ax}$ and $RX = \sqrt{2ax - x^2}$. The arc in question is $RX \sin^{-1} AH/RX = \sqrt{2ax - x^2} \sin^{-1} \sqrt{a/(2a-x)}$. Not surprisingly, Gregory gives up the prospect of integrating this over x !

However, by December he had devised an alternative approach¹⁰. Using the fact that the surface cut from a hemispherical surface by two planes parallel to its base and two perpendicular to the base through its axis is equal to the surface cut from the circumscribing cylinder by the same planes, he was able to equate the spherical surface to a cylindrical one. Both Huygens and Leibniz had also used this fact in their analyses. Thus, instead of summing the arcs RS over the spherical surface, he could sum their projections, that is, the lines HS , or AH , on the cylindrical surface. He now wanted the sum of these sines, corresponding to each point of the periphery $ACBD$, i.e. $4 \int_0^{\pi/2} a \sin d(a\theta) = 4a^2 \int_0^{\pi/2} \sin d(\theta) = 4a^2 = AB^2$. The paper concludes with some remarks on the sine-curve which is thus produced.

Gregory was clearly very concerned with the impression this paper would produce. Among his manuscripts there are three drafts of it, all dated December and January, 1693-94¹¹. Each is carefully reworked with many alterations to grammar and wording, yet in all essentials they are the same paper. The published version, too, contains further unimportant variations. It was sent to Halley, then clerk to the Royal Society, from Oxford on 11th January, 1694 and

¹⁰ C137.

¹¹ C137, C67, C97.

read to the Society on the 17th of that month, before appearing in the January Transactions¹².

The paper was, like the best of Gregory's work, competent, lucid, elegant and basically elementary. The indivisible techniques used were already somewhat old-fashioned (although quite sufficient for this problem) and Gregory was again proving a previously known result. Nevertheless, it is a pleasing paper, and must have done him credit in the eyes of the Royal Society, which was his main aim in writing it.

Six years later, Gregory wrote in the Transactions on the associated problem of Hippocrates' lunula, this time explicitly at Wallis' request¹³. As well as the paper itself, there are among Gregory's manuscripts further remarks on the lunula, by both himself and his brother James, mainly of 1693¹⁴. As early as 1690, Laurence Oliphant's graduation speech at Edinburgh, copied by Gregory and probably written under his influence, discussed the quadrature of the parts of the lunula¹⁵. However, this problem too, was essentially elementary, solved by standard (though elegantly presented) geometrical techniques.

Gregory was pleased with his work on these two problems. In a note, probably of 1694, on his hopes of publishing the Notae, he remarks on his intention to publish with them, inter alia, a theorem on the quadrature of this lunula¹⁶. In January, 1704, when planning a general

¹² RS L130 sup 4.31; RS Cl.P. 1.21;
PT XVIII (January, 1694) no 207 25-9.

¹³ PT XXI (December, 1699) no 259 411-18.

¹⁴ C190; C64.

¹⁵ C190; see Chapter 2.12.

¹⁶ C42.

text-book on the calculus, Gregory wished to include these topics. He intended to publish his quadrature of the lunula and his proof of Viviani's solution alongside the work of Cavalieri Torricelli, Viviani and Gregory of St Vincent on indivisible methods¹⁷. Although his enthusiasm for fluxions was soon to divert Gregory's attention away from such techniques, he did not abandon them altogether.

However, Gregory cannot have been unaware that Newton was in possession of techniques far in advance of anything Wallis or Huygens could teach him. This period of Gregory's life saw the completion (insofar as he was then able) of the Notae, the publication of his first paper in the Transactions and his visit to Christian Huygens. But without Newton's help Gregory was able to make no progress in his mathematics and it was essentially a sterile period. May, 1694 was to change all that.

¹⁷ C1742

4.2 May, 1694; Reconciliation

Newton's surprisingly affectionate friendship for the nervous, brilliant young Swiss, Fatio de Duillier, has been discussed by Frank Manuel¹⁸. Whatever its psychological basis, this was a close relationship between the two men and was particularly so in the early 1690s. Gregory had known of their friendship while he was still applying for the Oxford professorship, for in one of his letters to Newton at that time he informed him of Fatio's return to England¹⁹.

In the years when he was out of favour with Newton, Gregory met Fatio several times. They appear to have first met in December, 1691, and thereafter Gregory visited him whenever he was in London. In particular, they met when Gregory was on his way to Holland in 1693, and a year later in March and April, 1694. The last of this series of meetings came in May, 1694²⁰. Their friendship continued for many years, but they were apparently never again so intimate as at this time. Gregory seems, like Newton, to have dropped the acquaintance after the scandal of the Cévennes prophets in 1707. However, it must be pointed out on Gregory's behalf that he was extremely busy in this last year of his life, and the absence of Fatio's name from his notes may simply indicate a preoccupation with other affairs.

When Gregory and Fatio met, their main topic of conversation was Newton and his work. They discussed his latest researches and the

¹⁸ Frank E. Manuel. A portrait of Isaac Newton (Cambridge, Mass., 1968) 191-212.

¹⁹ Gregory to Newton: 10.10.1691 NCIII 372 169-70.

²⁰ C86, 27.12.1691; C76, 1693; A37, 31.3.1693; C64, 23.3.1694; C55, RG fo 79, 10.4.1694; C52, RG fo 76, May, 1694.

difficulties in the Principia. Gregory, barred from Newton himself, was apparently using Fatio as a substitute in his endeavour to absorb the Newtonian science. Finally, though, in May, 1694, Gregory was to be readmitted to Newton's favour. It seems most likely that this reconciliation was effected through Fatio de Duillier.

According to the book of exits and redits for Trinity College, Newton remained in Cambridge throughout 1694²¹. Certainly Gregory does not record any meeting with him between December, 1691 and May, 1694, and it is hardly likely that such a meeting would have gone unremarked by him. It cannot, then, have been a casual meeting in London which brought them together again.

It is, of course, possible that Gregory wrote to Newton asking him to explain the problems he had met in the Principia, but there seems no reason why he should have done so now, and not two years before. If there was such a letter, it is now lost, and in any case, Gregory's friendship with Fatio would at least have helped to influence Newton as to the tone of his reply.

Eighteen months earlier the hypochondriac Swiss, who lived for a further sixty years, had believed he was dying. In his stead, he offered Newton his brother as disciple. Perhaps some similar impulse led him to effect the reconciliation with Gregory, or perhaps he merely enjoyed showing the Scot how much his patronage might be of use. We can only conjecture as to Fatio's motives, but the coincidence of two meetings with Gregory in March and April with another in May following Gregory's visit to Cambridge cannot be overlooked. Whether

²¹ Correspondence of Sir Isaac Newton and Professor Cotes ...
J. Edleston (ed.) (London, 1850) lxxxv.

or not there was a lost letter from Gregory applying to Newton for help. Fatio's influence must have been an important factor in whatever events led up to the May meeting.

This May meeting was to dramatically alter Gregory's scientific work and professional life. Later meetings were to take place between them, but none would have an impact equal to that of the five days Gregory spent in Cambridge from the 4th to the 8th of May, 1694.

He made many notes on the talks he had then with Newton, which covered a wide range of topics from astronomy, mechanics, physics and mathematics²². The mathematical topics included the radius of a conic, the conjugates of mechanically produced curves, the determination of the polar co-ordinates of an orbit, and the form of the solid of least resistance. Newton told Gregory that

'The problem of quadratures and the inverse method of tangents includes the whole of more advanced geometry'²³.

Laurence Oliphant's graduation speech, on which Gregory had had a strong influence, had pointed out the importance of this problem of quadratures or integration, for physics. In this context he mentioned particularly the work of Huygens and Newton²⁴. Now Gregory heard from

²²C33, RG fo 65 NCIII 441, 311-15.
 C40, King's College, Cambridge, NCIII 443, 326.
 C43, RG fo 68 NCIII 444 327-28.
 C43, EUL, NCIII 445 331-32.
 C44, RG fos 68, 9. NCIII 446 334-36.
 C45 RG fo 69 NCIII 447 340-42.
 C57 EUL NCIII 448 344-45.

²³C33 RG fo 65,

²⁴C190.

Newton that

'All the more difficult problems are solved by squaring or by drawing tangents to a curve with a given property, or by finding the nature of the curve from a given property of its tangent'²⁵.

The importance of these problems was thus emphasised for Gregory. Further, he learnt much from Newton on the manner in which these problems might be solved. It was the tract 'De Quadratura', which his own work had prompted Newton to write. Indeed, one imagines Newton might have made a point of showing him this tract in order to emphasise how much advanced his work was beyond the abrupt series, of which Gregory seems to have continued to consider himself co-author. In this tract, Gregory said

'[Newton] develops that matter astonishingly and beyond what can readily be believed'²⁶.

Gregory would already have been familiar with the excerpts from this tract which appeared in the 1693 edition of Wallis' Algebra, but the full manuscript went far beyond these excerpts²⁷. Before this visit to Newton, Gregory did not use either dot notation or the terminology of fluxions. Afterwards, of course, he used nothing else. The problems which Gregory had met in the Principia were also discussed,

²⁵ C43, RG fo 68.

²⁶ C44, RG fos 68, 9.

²⁷ The tract is reproduced in MP VII pt 1.

and on 8th May, Gregory first proposed to Newton that his Notae be published²⁸. He also noted various alterations which Newton intended to make in any new edition. Out of all Gregory's problems, however, it was the solid of least resistance with which Newton gave him most help. Among Gregory's papers there is a manuscript by Newton on the truncated cone which undergoes least resistance, which was probably given to him during this visit²⁹. Two months later, Newton wrote with the general solution, and this was one of the problems which Gregory used to illustrate his 'Tract on Fluxions'³⁰.

After this meeting, Gregory slipped effortlessly under Newton's influence. As far as his scientific work went, this influence was almost total, and it extended to other spheres as well. Three traits which were to characterize their relationship are already apparent in this meeting; Newton advised Gregory on his work, told him of the progress of his own and enlisted his help in various tasks.

One of the subjects raised was Gregory's forthcoming text-book on Optics. Newton advised him on its contents, just as he was to do in a far more detailed and comprehensive way, for Gregory's Astronomiae. Newton was also to discuss many of the details of Gregory's edition of Euclid, and the projected one of Apollonius.

Gregory was also shown an early manuscript of Newton's which contained the foundations of his later work. He heard Newton's view

²⁸ C42.

²⁹ C34, RG fo 165 NCIII 442 323.

³⁰ Draft of Newton to Gregory: 14.7.1694 NCIII, 460 380-82. Gregory's use of this letter discussed in Chapter 4.3.3.

of Hooke's claim to priority in the concept of universal gravitation, and duly inserted it in the Notae, along with many of the changes Newton wished to make in a second edition of the Principia. The explanations of problems in this work and the tract 'De Quadratura' brought Gregory up to date with Newton's mechanics, astronomy and mathematics. Thus, as was to continue in the future, Gregory was told of Newton's early work, and especially his priority in various fields, he had the recent work explained to him, and was told of Newton's future plans for publication.

Shortly after Gregory's visit, Newton was asked to comment on a proposed plan for teaching mathematics at Christ's Hospital³¹. Gregory was thus able to perform his first task for Newton when, on 13th June he drew up a paper comparing the old and new plans. This paper went under the joint names of himself and John Wallis, but was drafted by Gregory, and Newton later thanked him for his trouble, thus making it a personal favour rather than a service to Christ's Hospital³². In the sameway in the future, when Newton wanted a reliable man to sit on the committee dealing with Flamsteed's stellar observations, or some-one to oversee the Scottish Mint, he would call on David Gregory.

The Scottish mathematician was delighted with this new relationship. The frequent repetition in his Notae of such phrases as 'ut auctor saepissime mihi dixit' (As the author has most frequently told me) and the faithful recording in his notes of Newton's plans show us that Gregory was, understandably, proud of his position as Newton's

³¹ Op cit²¹ 280.

³² RG fo 90. Newton to Gregory: 14.7.1694 NCIII 460 380-82 380.

confidant. However, he must justify such a position, whether to himself, to Newton or to the world. With Newton's reluctance to go into print, and his frequent obscurities when he did so, the work of disseminating the Newtonian philosophy was waiting for Gregory to take up, and, to the limits of his abilities, he did so. This role would not have suited Fatio de Duillier who boasted of how he had independently discovered Newton's doctrine of prime and ultimate ratios, and that his understanding of the calculus went far beyond Leibniz's³³. However, David Gregory readily acknowledged Newton's superiority to him in all matters, and, to him, the part of Newton's popularizer was an honour. It was not until the publication of his Astronomiae in 1702 that David made any real impact in this role, but his first efforts were directed towards Newton's mathematics, and to this end he wrote his 'Tract on Fluxions'.

³³ Fatio to Huygens: 15.2.1692 NCIII 383 193-94.
C52 16.5.1694 RG fo 76 NCIII 450 355.

4.3 The Tract on Fluxions

On 7th September, 1694, David Gregory drew up a draft for his 'Isaaci Newtoni Methodus Fluxionum, ubi Calculus Differentialis Libnitii, et Methodus Barrovii explicantur, et exemplis quamplurimis omnis generis Illustrantur'³⁴. Based, of course, on Newton's methods, it would also consider those of Leibniz, which he would show was an equivalent system. In this way, Leibniz's unproven method would be proven from Newton's proven one. Barrow's method of tangents would also be presented as an equivalent method from which the others might be deduced. This was to be followed by examples of the use of fluents and their fluxions in drawing tangents and finding areas. Further, more difficult problems would be divided into those concerning tangents, areas of plane and curved surfaces, maxima and minima, points of inflexion, the measurement of curves and curvature. These would include the solid of least resistance, evolutes and causticae (the curves determined by the intersection of rays from a given point reflected or refracted by a given curve). The inverse method of tangents would be illustrated by an example Newton had given him - the path followed by a weight pulled by a constant string whose other end follows a straight line³⁵. What Wallis had published of Newton's quadratures was to be included, especially the reduction of non-quadrable curves to the quadrature of a conic. Finally he would consider other problems of

³⁴ 'Isaac Newton's method of fluxions, where Leibniz's differential calculus and Barrow's method are explained and illustrated by very many examples of every kind'. Original MS, SUL MS QA33 G8 D12. The draft is C79, 7.9.1694 RG fo 64 NCIV 471 15-16.

³⁵ C43 5-7.5.1694 RG fo 68 NCIII 444 327-28.

resistance, and the best positions for sails and rudders.

Many of his examples were to come from the Bernoullis. Huygens on evolutes, and Tschirnhaus on these, tangents and maximisation are also mentioned. However, the primary purpose is the explanation and illustration of Newton's method of fluxions.

The tract itself was written in the following autumn, and bears dates from 23rd October, 1694 to 3rd January, 1695. It follows the above draft fairly closely, except that some of the proposed examples are omitted. Newton's reduction of the quadrature of intractable curves to that of conics and his example for the inverse method of tangents are not mentioned. Nor are causticae included. However, the outline of the tract is that of the draft plan.

4.3.1 The theory underlying fluxions

The extracts from 'De Quadratura' which appeared in John Wallis' Algebra were the first publication of the Newtonian 'dot' notation, which had appeared in Newton's papers only months before³⁶. They also gave precise definitions of fluents and fluxions; the fluents, x , y say, are 'indeterminate quantities, that is, those which, in the generation of curves, are continually increased or decreased by local motion'. Their fluxions, \dot{x} and \dot{y} , are 'the speed of increment or decrement' (my italics), and not the increment or decrement itself. In order to prove his rules for determining the equation involving fluxions from that involving fluents (i.e., for differentiating) Newton introduced an indefinitely small quantity o . Then he defined $o\dot{x}$ and $o\dot{y}$ as 'synchronous moments or momontaneous increments' of the

³⁶ John Wallis Opera II (Oxford, 1693) 390-96; see MPVII 174 n 12 for Newton's use of dot notation.

fluent quantities x and y . Thus at the next instant in time, x and y will be $x + o\dot{x}$ and $y + o\dot{y}$. By considering $f(x+o\dot{x}, y+o\dot{y}) - f(x,y)$, neglecting terms in o^2 , he verifies that, in particular examples at least, the increment in $f(x,y)$ is that which his rules predicted.

In modern terms, we would introduce a time factor t . \dot{x} , the speed with which x increases is then, in more familiar notation, dx/dt . Clearly o is nothing other than dt so that the 'momentaneous increment' of x is $dt \times dx/dt$, or dx . Clearly this \dot{x} is a very different concept from Leibniz's dx , which corresponds more nearly to our own dx . This difference would be emphasized by both Newton and Leibniz in 1712, in Leibniz's review of the 'De Analysi' and Newton's reply to this review³⁷.

However, Gregory shows in proposition 1 of the 'Tract on Fluxions' that he has not grasped this distinction. Fluents are still indeterminate quantities, subject to continual change, but for Gregory fluxions were the changes themselves and not their speeds. He is careful to emphasise that the fluxion of both ordinate and abscissa are made in the same minimal time, and so, if we take this time as the unit by which we measure speed, his definition is equivalent in practice. Nevertheless, the definition which Gregory gives here, in an explicit attempt to expound Newton's methods, is not the Newtonian definition.

In fact, as Gregory points out, it is the definition of moment in his Exercitatio. This definition had taken moments as the change in ordinate corresponding to a unit change in the abscissa, and in practice

³⁷ MPII 259-73; see also MPVII 182 n 26.

Gregory took \dot{x} as a unit fluxion. Newton's tract 'de Methodis Serierum et Fluxionum', composed probably in winter 1670-71, had used the term 'moment' for the product $o\dot{x}$, that is, in much the same sense as Gregory had used it in the Exercitatio. However, he had always been careful to keep this concept distinct from that of fluxion. It seems that Gregory, with the best will in the world, was unable to free his mind from the concept which he had always used in such work. His way of assimilating the new Newtonian concept of fluxion was simply to equate it with his own familiar moment.

Nor was Gregory the only British mathematician to do this. Raphson, writing in 1715, again with the specific intention of expounding the doctrine of fluxions, defined fluxions as 'Increments or Decrements of a continued Motion', and not as the speed of increment³⁸. Others, such as George Cheyne, wrote on fluxions without defining them at all³⁹. This change of definition made little practical difference in the applications of fluxions theory to problems, but was central to the differences between the Newtonian and Leibnizian systems.

It also confused Newton's proof of his rules for differentiation. For now, having multiplied his fluents by the infinitely small o , Gregory could only repeat Newton's definition of $o\dot{x}$ and $o\dot{y}$ as synchronous moments. But fluxions had themselves been so defined, thus reducing o to a dimensionless coefficient, whose introduction is pointless. Also, when o is merely an arbitrarily small scalar, the justification for omitting terms in o^2 can no longer rest on the

³⁸ Joseph Raphson The history of fluxions (London, 1715) 5.

³⁹ George Cheyne Fluxionum methodus inversa (London, 1703).

(implicit) concept of o as a time unit. Thus the proof of this process as presented by Gregory must of necessity be somewhat less convincing than Newton's one.

Gregory goes on to tell us that he follows Newton's method, rather than that of Barrow, Tschirnhaus or Leibniz, both because only Newton's is proven and also since Newton had known it as early as 1664, because it ante-dates all the others. There is no suggestion in this tract that Leibniz might have developed his method from Newton's, but it is made quite clear that the latter has priority of invention. In May, 1694, Newton had made sure Gregory recognised his priority over Hooke in the matter of universal gravitation. It seems that he had emphasised his priority in other fields, too!

Bernoulli had said in the Acta that Barrow's method of tangents coincided with those of Tschirnhaus and Leibniz⁴⁰. Using this testimony, Gregory need prove only that Leibniz's and Newton's methods coincide, and he will have all four methods equivalent. Naturally, because of the way he has defined fluxions, Gregory finds that Leibniz's differentials are identical to them. By examining the rules given by Leibniz, and showing their coincidence with Newton's he concludes that the systems are equivalent. Thus Leibniz's method is proven since Newton's is.

The tract now moves on to the use of Newton's fluxions in physical and geometrical problems.

4.3.2 Examples of the use of fluxions

Most of the 'Tract on Fluxions' is taken up with copious examples

⁴⁰ AE (January, 1691) 14, (June, 1691) 290.

on their use. These have been taken mainly from the Acta, with two from Barrow, one of de l'Hôpital's, probably supplied by Huygens, one example derived from a problem of Halley's, and others from Newton. These last were mainly those discussed in May, 1694 and are examined in 6.3.3 below.

More theoretical basis was necessary for most of these examples, and first he examines drawing tangents. Having shown by similar triangles that $\dot{x} : \dot{y} = \text{sub-tangent} : \text{ordinate}$ he finds tangents to the higher parabolae and thus shows that Sluse's method is contained in that of fluxions. He had long ago 'proven' the equivalence of Fermat's and Sluse's methods by showing that they produced the same result when applied to specific examples (see Chapter 3). In the same way, he now works through two examples in Sluse's and in Newton's style arriving at the same answer in each case. As before, he asserts that this proves their equivalence, and, recalling his earlier result, both are equivalent to Fermat's method. Over ten years after his first abortive attempt to submit to the Royal Society, he was still satisfied with such an inconclusive proof.

Gregory gives several examples of finding tangents to curves, all results previously found by others. In particular, propositions 9 and 24 solve the first 4 problems of lecture 10 in Barrow's Geometrical Lectures, and proposition 15 finds the tangent to the helicoidal parabola given by James Bernoulli. Their calculations are recast in fluxional notation, but Gregory adds nothing to their analyses.

The problems on maxima, minima and points of inflexion are more

interesting. We know Newton showed Gregory his 'De Quadratura' in May, 1694, but this tract barely mentions these topics. However, they had been dealt with quite fully in the earlier tract 'De Methodis Serierum et Fluxionum'⁴¹. This explained how to equate the fluxion to zero to find a limit point, solved two such examples and suggested many more. Gregory had a paper in his possession consisting of extracts from the 'De Methodis'. Probably, as Whiteside suggests, these extracts were a summary or a copy of notes John Craige had made on Newton's work in 1685⁴². However, they are concerned with infinite series and omit the methods of maximisation contained in the tract.

Most probably, Gregory was shown the whole tract in May, 1694, and then learnt of Newton's methods of maximisation. The example Gregory gives in the 'Tract on fluxions' of finding points of inflexion, Nicomedes' conchoid, was the very example Newton used in the 'De Methodis', which supports the suggestion that Gregory had then seen this tract.

Moreover, Gregory follows Newton in determining this point by finding the limit value of the tangent's intercept, although taking this intercept with the other axis from that taken in the tract. In fact, this method gives a correct value in this particular example but can by no means be universally applied. Newton's tract does not mention the alternative (and universally applicable) method of equating the second derivative to zero. However, Gregory does so, and shows that this is an equivalent method in the case of the conchoid. He may have

⁴¹ MPIII 38-328.

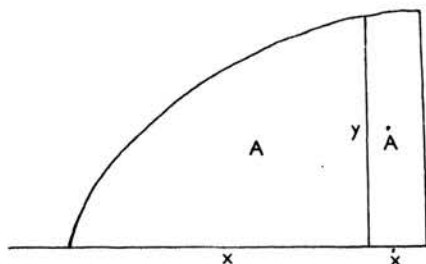
⁴² A56; MPIII 354-55 nl.

known of this method from the writings of Leibniz and Bernoulli in the Leipzig Acta, or Newton may have discussed it with him privately.

In any case, it seems that Gregory, who already had obtained extracts from it through John Craige, was shown the full tract 'De Methodis' in 1694, perhaps as much for further evidence of Newton's priority as for the help it might be to Gregory who also saw the much later 'De Quadratura'. At the very least, Newton used the example of this tract to explain points of inflexion to Gregory and probably also discussed maxima and minima with him.

As far as I can discover, no-one at this time was using the second derivative as a criterion to distinguish between maxima and minima, and Gregory was no exception here. Occasionally he appeals to geometric considerations to make this distinction, but generally he assumes that the limit point he has found is of the desired kind. ~~More surprisingly, in his many examples of such problems, he consistently ignores the solution $x = 0$, which generally corresponds to the limit point of the opposite kind to that for which he is looking. In a typical example (proposition 32), he requires the maximum cylindrical surface which can be inscribed in a given sphere of radius r . If x is the radius of any inscribed cylinder, its surface is proportional to $(r^2 - x^2)^{\frac{1}{2}}$, giving the equation $\frac{2r^2 x \dot{x} - 4x^3 \dot{x}}{2(r^2 - x^2)^{\frac{1}{2}}} = 0$ at a limit point. Here we have two solutions; $x = 0$ for the minimum surface, and $r^2 = 2x^2$ for the maximum one. However, Gregory totally ignores the former solution and considers only the latter.~~

By considering the quadrature of the higher parabolae, Gregory introduces the fundamental theorem of the calculus. If we consider an infinitesimal increment \dot{A} of the area A below a curve, corres-



ponding to an increment \dot{x} of the abscissa, we can say $\dot{A} = \dot{x} \times y$ (where y is the appropriate ordinate). Thus to find the total area under the curve, we sum over the increments $y \times \dot{x}$, that is, find the fluent whose fluxion is $y \times \dot{x}$. Here we have the opposite process to that of finding tangents, where we want to find the fluxion of a given fluent.

So, if $px = y^2$, and we let the area under the curve be $A = dx^e$, we have $\dot{A} (= y \times \dot{x} = p^{\frac{1}{2}}x^{\frac{1}{2}} \times \dot{x}) = dex^{e-1}\dot{x}$. This gives $e = 3/2$, $d = \frac{3}{2}p^{\frac{1}{2}}$ and the area is $\frac{3}{2}p^{\frac{1}{2}}x^{\frac{3}{2}}$. Similarly, the area below $y = mx^{p/r}$ is $\frac{r}{p+r} mx^{(p+r)/r}$, which, as he is quick to point out, is the very rule given in Gregory's Exercitatio.

To illustrate the inverse method of tangents, he takes the curve whose intercept is given, that is, where $y = b\dot{y}$ (\dot{x} is constant, say $\dot{x} = 1$). He argues that, since quantities which are as their differences are in geometric proportion, we have a curve where the ordinates corresponding to an arithmetic progression of the abscissa are in geometric progression. That is, the required curve is logarithmic. This property, that the sub-tangent of the logarithmic curve is constant was frequently used by Gregory, almost as the basic definition of the curve, especially in the form $y\dot{x} = b\dot{y}$. (Of course, Gregory's logarithmic curve is not, as we would expect to-day, $y = b \log \frac{x}{a}$ with y the ordinate and x the abscissa, but $x = b \log \frac{y}{a}$. This curve was to be crucial in Leibniz's analysis of the catenary which Gregory studied later.)

The remaining examples came largely from the Leipzig Acta and were typical of the work Gregory was to do in the following years in

his workbook E. In particular, he took two papers which James Bernoulli had published in the Acta in 1691, and used these in full⁴³. Bernoulli's work on the helicoidal parabola, which included the determination of tangents, area and points of inflexion was translated by Gregory into the notation and terminology of fluxions. His work on the basic determination of evolutes and the measurement of the log spiral was similarly treated, providing in all almost a quarter of the examples used. He also used a paper of Bernoulli's from the 1693 Acta to produce two examples on the resistance of solids⁴⁴. Gregory proved the results stated by Bernoulli, and another, apparently of his own, by applying the principles of resistance used by Newton in the far more difficult case of the solid of least resistance.

Huygens also provided Gregory with some of his material. Foliate curves had been one of the subjects discussed between them in 1693, and Gregory examines these curves fully in the tract. More importantly, Gregory owed his rectification of the logarithm to Huygens. To rectify the curve $y = r \log x$, we must evaluate $\int_{x_2}^{x_1} (1+r^2/x^2)^{\frac{1}{2}} dx$. On substituting $z = \sqrt{x^2+r^2}$ and resolving into partial fractions we arrive at the result

$$s = \sqrt{(x_2^2+r^2)} - \sqrt{(x_1^2+r^2)} + r \log((x_2^2+r^2)^{\frac{1}{2}}-r) - r \log((x_1^2+r^2)^{\frac{1}{2}}-r) \\ - r \log x_2 + r \log x_1.$$

This was naturally expressed in the late seventeenth century by a geometrical construction involving the logarithmic curve itself.

James Gregorie had solved the problem in 1670, and sent his

⁴³ AE (January, 1691) 12-23, (June, 1691) 282-90.

⁴⁴ AE (June, 1693) 244-56.

solution to John Collins on 19th December of that year⁴⁵, but it was not to this infinite solution, which he had used in his Exercitatio, that David turned. On 10th September, 1692, de l'Hôpital had sent his solution to Christian Huygens, who described it in his private notes as 'admirably beautiful and subtle'⁴⁶. The Dutchman was indeed most impressed by the solution and, over the next four months, spent much time reworking it and filling in the details of de l'Hôpital's derivation⁴⁷. Eventually he was able to refine the geometric expression somewhat, and he then wrote to de Beaval, sending the solution (duly attributed to de l'Hôpital) for insertion with various things of his own in l'Histoire des Ouvrages des Scavans, where it appeared in February, 1693⁴⁸.

However, Gregory's solution of the problem in his 'Tract on Fluxions' does not include Huygens' refinement - that is, it is taken directly from de l'Hôpital's (then unpublished) letter to Huygens, and not from the article in the Histoire. Gregory's proposition 41 takes over its proof structure entirely from de l'Hôpital, merely translating it into the language of fluxions. However, as he could usually do in such cases, he adds something to the original by elaborating each step of the proof.

Gregory was aware of the printed article, though possibly not when he first wrote his 'Tract'. He inserted a later note into his

⁴⁵ James Gregorie to John Collins: 19.12.1670 GTV 148-50.

⁴⁶ Huygens op cit⁸ X 314-15; ibid XX 547-50.

⁴⁷ Ibid X 314-17 passim.

⁴⁸ L'historie des ouvrages des scavans (February, 1693) 244-57.
Huygens op cit⁸ X 407-17.

original manuscript, referring readers to his manuscript C171 for the synthetic construction of this problem. This item is unfortunately now missing, but its title in the index refers to the Journal (a mistake for l'Histoire des Ouvrages) des Scavans for the basis of this paper.

Yet Gregory's proposition was based on de l'Hôpital's letter. Clearly, when visiting Huygens in 1693 (when he had made a resolve to enquire about de l'Hôpital's work⁴⁹) he had seen, and perhaps copied, this letter. Indeed, judging solely by Huygens' enthusiastic response to this paper, he would have been likely to have shown it to his young Scottish visitor.

Another problem in this tract shows the importance of Gregory's personal contacts, this time with Edmond Halley. Problem 40 deals with rotation of a wheel, and defines the point at which a given force should hit a given wheel (or globe) to produce a maximum rotation. Gregory deduces what proportion of the given force will be used to produce rotative (rather than translative) motion and then maximises it.

Gregory had been interested in problems of rotation for some years. One of the earliest notes after his move from Edinburgh to London, in December, 1691 discusses the quantity of motion in bodies rotated about their own axis⁵⁰. At that time he met Robert Boyle, and the question had been raised in their discussions, possibly providing the impetus for this paper⁵¹. The problem was especially

⁴⁹ A8.

⁵⁰ C177, London, December, 1691 RG fo 86.

⁵¹ C86 25.11.1691.

important for Newton's study of the precession of the equinox and Gregory used this earlier paper for his Notae on this point, explaining that as rotation has only recently been understood, it is better to begin its discussion from first principles. These notes were made around 8th December, 1693⁵².

Now on 19th January, 1695, just as Gregory was completing the 'Tract on Fluxions', Halley proposed to him the problem of finding the rotary motions produced in two spheres by an oblique impact⁵³. This, of course, is essentially Gregory's problem of a given force hitting a sphere obliquely. Two further papers among David Gregory's manuscripts deal with the problem. One, undated, is titled by Gregory as a problem of Halley's, and, in another hand, gives the solution to this problem. The other is partly dated 12th February, 1695 and is Gregory's solution, built on some basic lemmas and including a reference (possibly after the date given) to proposition 40 of the 'Tract on Fluxions'⁵⁴.

It seems that Halley, having proposed the problem, gave his solution to Gregory, but we cannot tell whether this was before or after Gregory had produced his own. The similarity of this sheet of Halley's to the solution to proposition 40 is marked, but the problem is not very abstruse, and the two men may well have arrived independently at the same approach.

Whether or not Gregory used Halley's solution in the tract, this

⁵² Isaac Newton Philosophiae naturalis principia mathematica (London, 1687) bk 3 lemma 2, 469.

⁵³ A40 RG fo 96 NCIV 82 nl.

⁵⁴ A7; A25.

example certainly arose out of the problem he had suggested. Both in this tract and elsewhere, too, Gregory spent quite some time and effort on developing his solution.

The importance of these influences of Huygens and Halley is immediate. Of course, theirs were not the only influences on the tract besides Newton's but Newton might himself have suggested the study of the Acta (although it is more natural to attribute any such suggestion to Huygens). Even if he did not in fact suggest it, Newton would certainly have approved Gregory's study of Barrow. However, in Huygens and (to a lesser extent, since he, too, was a disciple of Newton) in Halley, Gregory was susceptible to other influences than Newton's. As his life continued, these other influences were to become less important, but they never entirely disappeared. Even Newton's sway was not absolute.

4.3.3. The use of Newton's examples

As we have seen, Gregory used Newton's 'De Quadratura' and at least some of the 'De Methodis' to establish the definitions and basic methods to be used in his tract. In addition, four of the examples derive from Newton.

The first of these is proposition 14. Given two points in media of different densities, and the plane surface dividing the media, determine the point in this surface such that a body crossing from one given point to the other through two straight lines joined at this point does so in the least time. This approach to the problem of refraction was not new; Fermat had previously tackled it in this way⁵⁵.

⁵⁵ J.F. Scott The scientific work of René Descartes (London, 1952) 40.

Newton's contribution, in a paper which he gave to David Gregory, was to restate and solve the problem in terms of fluxions, with a great attendant simplification of the calculation⁵⁶. Gregory's contribution was, as usual, to fill in the details of the argument. He also emphasized that this result (which leads directly to the familiar $\sin(i) \propto \sin(r)$) was one of many examples of Nature acting in the quickest way. It was no doubt a continuation of this line of thought which led to Gregory's abortive attempt in 1697 to show that atmospheric refraction bends a light ray into the form of a cycloid, which he then knew was the curve of quickest descent⁵⁷.

We do not know precisely when Newton gave Gregory this paper on refraction, but the next example arose directly out of their discussions in May, 1694. Newton had then told Gregory his theorem for finding the centre of curvature of a conic, and a proof of it is also among Gregory's papers⁵⁸. Proposition 25 of the 'Tract on Fluxions' considerably simplified this theorem by considering curvature at the vertex only, but it is clearly derived directly from it.

The other Newtonian examples are two propositions on solids of least resistance. In the Principia Newton stated without proof the constructions for the frustrum and the general solid which suffer least resistance when moved quickly through a fluid in the direction

⁵⁶ C38 RG fo 165. Gregory also used this example in his Opticae; see 2.6.3.

⁵⁷ A100 see Chapter 5.3; this point is amplified in 6.3.

⁵⁸ C33 RG fo 65 NCIII 441 311-5; C45 RG fo 69 NCIII 447 340-42.

of their axes⁵⁹. This scholium had been one of the major problems encountered by Gregory in his study of this work. He had raised the question more than once in his discussions with Fatio de Duillier in 1693/94, and it figured in the list of queries which he intended to raise with Newton at the May meeting⁶⁰. The problem was discussed between them, for Gregory noted down the quantity, deduced from physical considerations, which must be minimized in the case of the frustrum⁶¹.

However, Newton must have realised that this was not enough for Gregory. He also gave him two drafts of the calculation for the frustrum and wrote to him in July, 1694, sending a reworked copy of the general case⁶². The particular case of the frustrum is relatively straightforward if we accept Newton's physical assumptions, and Gregory made good use of it. He inserted it in his *Notae*⁶³, and used it to provide proposition 26 of the 'Tract on Fluxions'.

The general case was considerably more difficult, and Gregory was not the only one of Newton's contemporaries to be baffled by it. Huygens alone seems to have been able to reach the required solution, and even Leibniz could only note beside the scholium '*Investigandum est isoclinis facillime progrediens*'⁶⁴.

⁵⁹ Newton op cit⁵² bk 2 scho. to proposition 35 326-27.

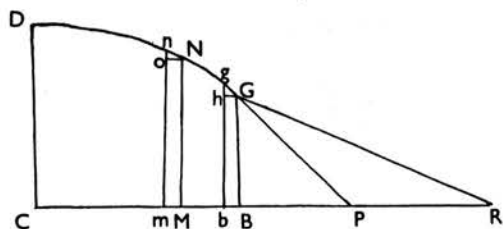
⁶⁰ A37, 31.3.1693 and C64 23.3.1694; Misc. 2.

⁶¹ C43 RG fo 68 NCIII 444 327-28.

⁶² C48 in private possession, cited MPVI 471; C43, RG fo 165 MPVI 470-71; UCL Add. 3967-72: 10^r - 11^r MPVI 475-77.

⁶³ Notae 87.

⁶⁴ See MPVI 446 n25.



The problem is: given two lines DC, GB, find the curve DNG by whose revolution about CB a solid is formed which encounters the least resistance on moving

quickly through a fluid in the direction of CB.

In the solution which he sent to Gregory, Newton considered two infinitesimal portions of the arc, nN and gG , where the increments no and gh are equal. The resistance on each piece is as $\frac{1}{n}N^2$ and $\frac{1}{g}G^2$ respectively, or, when we consider the infinitesimally wide ring making up the solid of revolution, as MN/nN^2 and BG/gG^2 . Minimising $MN/nN^2 + BG/gG^2$, Newton arrives at $Gg^4:Nn^4::BG \times Bb:MN \times Mm$. Further, if we draw GR parallel to the tangent at N and then let $gh = hG$ (justified by the previous work on the frustrum) we deduce that $4BG^3 \times BR:GR^3::GR:MN$ gives the required solid, as was stated in the Principia.

Whiteside points out that there is no mathematical gain in making MN and bg distinct, and in an earlier draft Newton had allowed them to coincide. However, Whiteside suggests, separating the two lines

'serve the heuristic purposes of allowing the unsophisticated reader - and Gregory in particular - more readily to accept the accuracy of Newton's infinitesimal approximations, according to which no distinction need be made between BG and bg or MN and mn, but the second-order difference $no - gH$ is of crucial significance'⁶⁵.

⁶⁵ MP~~4~~VI476 n31.

However, in spite of Newton's care, Gregory does not seem to have grasped the necessary distinction of BG and MN. Following the physical argument of the frustrum case, he deduces that the resistances on gG and Nn are as $1/Gg^2$ and $1/Nn^2$. He then, however, ignores the fact that they are part of a circular solid, and proceeds to minimise $1/Gg^2 + 1/Nn^2$. Thus he arrives at the formula $GR:4BR::BG^3:GR^3$.

It is difficult to see how Gregory could have been satisfied with this answer. It differs from the Principia and from the draft of Newton's letter. The answers can only be reconciled if we allow BG to equal MN (which would also justify Gregory's choice of quantity to minimise) but this would, of course, be an unjustifiable assumption. Indeed, as shown above, Newton seems to have taken pains to let Gregory see that such assumptions cannot be made. Unfortunately we do not have the actual letter sent to Gregory, but only Newton's draft. However, if we except this point, the 'Tract on Fluxions' follows exactly the analysis of the draft letter. It also reproduces the figure exactly in so far as the distinct separation of the two infinitesimal elements.

Gregory must have been aware of some anomaly in his answer. He inserted the analysis of the case of the frustrum into a space left blank at the appropriate point in the Notae, but he never added any discussion of the general case⁶⁶.

We can only surmise that the letter which Newton eventually sent to Gregory was such that the latter was able to build this analysis

⁶⁶ Notae 87.

around Newton's sketch and believe he had accurately reconstructed Newton's thoughts. Nevertheless, it seems unlikely that he could have reconciled this analysis with one such as that contained in the draft letter. It is, though, even less likely that he could have reproduced the analysis to the extent which he did without a similarly detailed guide. Unless further evidence is found, the problem of reconstructing Gregory's interpretation of the solid of least resistance must remain a mystery.

There is one further point in the tale. In 1729, Andrew Motte added some 'Explications (given by a Friend)' to his translation of Newton's Principia⁶⁷. These discussed the 2nd corollary to proposition 91, on the attraction of the spheroid (another problem which defeated Gregory)⁶⁸ and the cases of the frustrum and general solid which undergo least resistance. His discussion of this last problem was apparently taken from Newton's letter to Gregory, and was not the erroneous one found in the 'Tract on Fluxions'.

Perhaps a copyist of the 'Tract' had been able to amend Gregory's errors, or perhaps Motte or his friend had done so. Possibly the actual letter which Newton sent had come somehow into Motte's hands. In any case, this published version had not been taken straight from Gregory's original 'Tract on Fluxions'.

⁶⁷ Isaac Newton The mathematical principles of natural philosophy by Isaac Newton. Translated into English by Andrew Motte 2 vols (London, 1729) 2 i-viii.

⁶⁸ C60, C63, C181, C42 contain Gregory's attempts on this problem which he could never resolve satisfactorily enough to include in his Notae. His attempts were blocked by his inability to integrate $(ax)/\sqrt{(bx^2+cx+d)}$.

The examples Gregory used in this 'Tract on Fluxions' give us a measure of its originality - and this was virtually nil. With the possible exception of some of the examples and limit values and some remarks on the optimum position of a rudder, all of them elementary, the examples are the work of others, redone where necessary in fluxional notation.

Nevertheless, we must remember that originality was not the aim of this 'Tract'. It was intended to expound Newton's methods, and in this it succeeds admirably. At times, Gregory's attention to explaining every step of his work may seem tedious, or be in danger of obscuring the basic proof structure; nevertheless these lucid recapitulations of the work of others are generally easier to follow than their originals.

We know of five copies of this tract. David's original in St. Andrews University Library and a fair copy in Christ Church College, Oxford, are the two I have used. St. Andrews also has a copy entitled 'Problemata Mathematica' and there is one in Cambridge made by John Keill and one in private possession made by William James⁶⁹.

It is impossible to estimate now how widely it was read. Gregory himself refers to it frequently, particularly in workbook E, but I have found no other references to it. Aimed probably at his Oxford students, it went far beyond anything he taught at Edinburgh, and one wonders how many of his students were able and willing to make the effort to understand and assimilate it. For any who did, however, it provided an excellent groundwork in the methods of

⁶⁹ SUL MS31010 is Gregory's original.

Copies: SUL QA33, G8D12, Ch.Ch. MS131, ULC Lucasian papers pkt 13 and private possession.

fluxions and an introduction to some of the work being done on the continent.

As an insight into Gregory's work; it provides us with a measure of the mathematical influences upon him. Newton's is the predominant one, but there were also the continental mathematicians whose work he studied closely. Huygens and Halley also influenced the choice of examples. When we study the use he made of Newton's work, we find first that he did not so much absorb the new concept of 'fluxion', but redefined it in terms of the 'moments' with which he was used to dealing. Secondly, although he used three of Newton's examples quite competently, he was unable to reproduce the analysis of the solid of least resistance, even after it had been spelt out for him.

Gregory was pleased with the tract and referred to it often. His desire to extend it led on to the mathematical work which was to absorb him over the next few years.

4.4 Further Applications of Fluxions

During the year immediately following the completion of the 'Tract on Fluxions', Gregory did little mathematics. His text on Optics was published in this year and in the summer, he finally married Elizabeth Oliphant. It was early in 1696 that he turned again to mathematics and began his workbook E⁷⁰. It was from this manuscript that Hiscock took most of the entries for his collection of Gregory's memoranda but it was not primarily the occasional diary it later became⁷¹. Soon after he had begun his entries, Gregory noted his resolve:

'As soon as possible, the Leipzig Acta are to be procured, so that from the writings of Leibniz, de l'Hôpital and both Bernoullis, their method may become clear and so that the doctrine of fluxions hitherto set down by me in papers may be extended. 23rd April, 1696'⁷².

For the next year, this was precisely what he did, and virtually all the major papers which appeared in the Acta from 1690 to 1695 are examined in this workbook. One of the papers on the logarithmic curve follows Isaac Barrow and uses his techniques. Otherwise the problems are dealt with in terms of fluxions.

However, the second part of his programme was never carried out. No additions were made from workbook E to the 'Tract on Fluxions',

⁷⁰ Ch.Ch. MS346.

⁷¹ W.G. Hiscock (editor) David Gregory, Isaac Newton and their circle (Oxford, 1937).

⁷² E9.

although most of the work could quite appropriately have been added in just the form in which was presented in the workbook.

The problems are in the style of the 'Tract'. Gregory provides proofs when necessary, but generally needed only to adapt the published proofs into the terminology of fluxions. At least he could usually take the basic physical principles from the papers in the Acta. Rarely did he extend a result beyond what had been previously published, and then only trivially.

The four major problems which he examined were caustic curves, isochronous curves, the curve of equilibration and the elastica⁷³. All these had been examined by the continental mathematicians, especially Leibniz, de l'Hôpital and the Bernoullis, who had published their solutions in the Acta⁷⁴. Many shorter studies are also found in this workbook and they deal with many different topics; basic equations in the theory of fluxions, evolutes, conic sections, the logarithmic curve, the cycloid and many others. Not all of these problems are dated, but most may be placed in 1696 or 1697, while later problems in this workbook are concerned with astronomical topics, or problems of the Ancient geometers.

A typical example of these studies in the calculus was the

⁷³ Caustics E17-27, Isochrones E39-43, curve of equilibration E53-62, elastica E65-71.

⁷⁴ Caustics, James Bernoulli AE (May, 1692) 207-13, and (June, 1693) 244-56.
 Isocrones. Leibniz AE (April, 1689) 195-98, and (August, 1694) 364-67; James Bernoulli AE (June, 1694) 276-80, John Bernoulli (October, 1694) 394-99.
 Curve of equilibration, Leibniz AE (April, 1695) 184-85; de l'Hôpital AE (February, 1695) 56-59; John Bernoulli AE (February, 1695) 59-65; James Bernoulli AE (February, 1695) 65-66.
 Elastica, see 4.4.1.

Elastica; here Gregory understood and reproduced John Bernoulli's arguments, adding some simple corollaries of his own, but did not even think of suggesting an alternative formulation of the problem.

4.4.1 The Elastica

In 1691, James Bernoulli proposed the problem of the elastica to the readers of the Leipzig Acta and intimated that he had solved at least the simplest case⁷⁵. However, so that others might try to find a solution, he would not yet publish his own work.

The problem is that of determining the curve into which a flexible beam is bent by an attached weight; a problem which Bernoulli claimed was even more difficult than that of the catenary on which the best minds in Europe had been bent over the last year (see 4.6).

Truesdell concurs in this judgment, and cites Huygens' comment that

'I have not dared to hope that one would come out with anything clear or elegant here, and therefore I have not tried'⁷⁶.

Bernoulli's solution had been promised for the autumn of 1691, but did not actually appear until 1694. It went, however, far beyond the simplest case⁷⁷.

First, he set down the theorem he described as 'golden', which gave a formula for the radius of curvature, $z = dx ds / d^2 y$ or

⁷⁵ AE (June, 1691) 282-90

⁷⁶ Huygens to Leibniz: 16.11.1691. C. Truesdell 'The rational mechanics of flexible or elastic bodies'. Introduction to vols X and XI of second series Leonhardi Euleri Opera Omnia (Turici, 1960) 88 n4.

⁷⁷ AE (June, 1694) 262-74.

down Bernoulli's basic lever principle, Gregory was able to supply a proof of the general geometric construction of the elastica for any given curve of elongations.

Some of Bernoulli's corollaries are given here, and supplied with proofs, though for others Gregory simply refers his reader back to the original in the Acta. His utilitarian attitude to such problems (common to most mathematicians of his day) leads him to concentrate most on the 'real' problem, in which elongations are proportional to stretching force. After deriving the differential equation Bernoulli had given for this case he continues to prove many of the corollaries.

There is little of Gregory in this work. He has supplied proofs and a few comments on special cases such as that of a constant elongation, irrespective of stretching force, which follow easily from the general one. His only contributions are as usual, the translation from Leibnizian to Newtonian terminology and a certain gain in lucidity. Clearly he understands this difficult piece of work himself, and has made it far clearer to the reader than was the original - especially to a reader versed in fluxions rather than differences. However, he takes over Bernoulli's principles and mechanical insights in their entirety and makes no attempt to discuss them.

Much can be said on the basic principles on which Bernoulli based this analysis. Truesdell points in particular to

'the tragic flaw of Bernoulli's conception, ... his vacillation between the one-dimensional elastic curve and the three-dimensional elastic beam'⁸¹

⁸¹ Truesdell, op cit⁷⁶ 93.

However, Gregory's mechanical insight was not high, and it was to fail him especially in his attempt to analyse the catenary curve. He could understand and use Bernoulli's arguments once they had been set out for him, but his understanding was not sound enough for him to search out his own alternative postulation of the problem.

Again, however, we must remember the purpose for which he was writing. These papers, like the 'Tract on Fluxions', which he wished to extend by them, were not intended to be original but explanatory, and within these limits they were competent and successful. However, by the winter of 1696-97, he felt ready to tackle some original work, and his two attempts at this, on the brachistochrone and the catenary, will extend our picture of his mathematical abilities.

4.5 The Brachistochrone

In June, 1696, John Bernoulli challenged the learned world through the Acta to find the brachistochrone, or curve of quickest descent⁸². Six months were allowed for this problem which was that of finding the curve through two given points, not in the same vertical or horizontal line, such that a body descending through it under gravity does so in the least time. At the end of six months, no completely correct solution had been received, and so, yielding to the persuasion of Leibniz, he extended the time limit by a further three months and had the challenge published in the Journal des Sçavans and the Philosophical Transactions. Copies were also sent to Wallis and Newton, and he added a second geometrical problem. Newton's solutions appeared in the Transactions for January, 1697⁸³.

Meanwhile, Gregory had been attempting his own solution. In September, 1696, the problem of the brachistochrone had been proposed to Wallis at Oxford 'ab Helveto quodam'⁸⁴. Dr Whiteside has identified this Helvetus as Johann Bernoulli's youngest brother Heironymus, who had also taken a private challenge to Varignon in Paris⁸⁵. We do not know when Wallis first told Gregory of this problem, but the Scot had begun to tackle it by December, 1696.

His early study of Huygens' Horologium Oscillatorium, and his recent work on the continental methods gave him a knowledge both of

⁸² AE (June, 1696) 269.

⁸³ PT 19 (January, 1697) no 224 384-89.

⁸⁴ E30.

⁸⁵ D.T. Whiteside, letter to author: 23.12.1975.

the laws of bodies descending through curves and of the techniques necessary to solve the problem. It was an ideal opportunity for him to show the scientific world his worth as a mathematician. Also, even more than with his work on the catenary (which he began in a similar spirit at this time) by solving this problem through the use of fluxions he would show the power of Newton's methods.

Unfortunately, he did not succeed. We have the papers in which he attempted to find the curve, and on the first page he has written

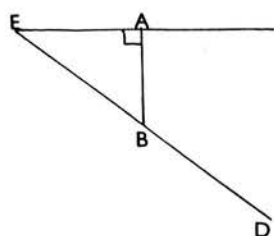
'22nd Feb. 1696/7 These papers are the traces of the calculations by which, in January and February 1696/7 I tried to show that the catenary was the line of quickest descent, but in vain, for the common trochoid is the required curve. But, on December 18th 1696, I suspected how the matter stands, namely that the trochoid is the line of quickest descent. But, other things interrupting, I left off its consideration. This appears from the sixth page of this inclusive'⁸⁶.

How was it then, that Gregory, with the wide mathematical knowledge and facility with the new techniques he had then acquired, not only failed to identify the curve as the cycloid, but believed he had shown it to be the catenary?

He began by considering descent under uniform acceleration through two contiguous, variously inclined planes.

Given AB, with mid-point D, and DE

through inclined planes, and still of bodies under uniform acceleration. He deduces that, after falling from A, the time of a body's



descent through BD is as $\sqrt{DE \cdot EB} - EB$.

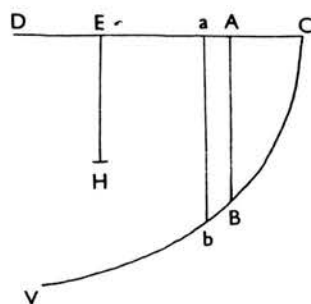
Then, considering the infinitesimally small arc of the required curve as though it were the line BD, he displays this

relationship in geometrical terms. Thus if δs is the infinitesimal increment of the curve which is expressed in terms of rectangular co-ordinates x and y whose origin lies on the horizontal line through A, x horizontal and y vertical, we have, for the time through δs ,

$$\delta t \propto y \delta s / \delta y (\sqrt{1 + \delta s/y} - 1).$$

(Gregory expresses this geometrically). We would have to sum this over x or y and then find a minimum of the integral. Clearly Gregory could go no further. He again mentions the cycloid, saying that we could check this theorem in the case of the cycloid if we showed that its integral was independent of the point at which we drew the horizontal line. (This, of course, follows from the isochronous property of the cycloid.) He does not appear, however, to have made any attempt at proving this himself.

Then, in early February, Gregory had what seemed to be a breakthrough. First, he discarded the unnecessary complication of regarding motion through an infinitesimal line as being uniformly accelerated. Now, with uniform speed, we can use time \propto distance/speed. Also, the



speed of a descending body is (as Gregory knew well) proportional to the square root of the distance through

which it has fallen. That is, the time taken to traverse Bb by a body which has fallen from C is as Bb/\sqrt{AB} . That is, calling CA x , AB y and CB s , we wish to minimise $\int_{x_0}^{x_1} \frac{ds}{\sqrt{y}}$. .

Unfortunately, Gregory at once saw a similarity between this expression, and that for the moment of the curve CBV about the axis CD ; $\int_{x_0}^{x_1} yds = EH \times CBV$, where H is the centre of gravity of the curve. Gregory argues that, as $\int_{x_0}^{x_1} yds = EH \times CBV$, so must $\int_{x_0}^{x_1} \frac{ds}{\sqrt{y}} = CBV/\sqrt{EH}$. Thus, the time taken to run through CBV is as CBV/\sqrt{EH} . Of course, such an argument is invalid. Gregory made at least one attempt to verify this result, for he tried to calculate the fluxion of CBV/\sqrt{EH} , but he was unable to do so. Nevertheless, he apparently had no doubts of the validity of this result. Indeed he was pleased with this theorem and repeated it several times in these papers.

But Gregory had also been working on the catenary over this period, and was familiar with its properties. In particular, of all curves of the same length, the catenary has the lowest centre of gravity, that is, EH is greatest. So, keeping CBV constant, we have CBV/\sqrt{EH} a minimum. Therefore, the brachistochrone is a catenary. Gregory was also pleased with this further result, and it, too, is repeated several times.

Clearly, his major error lay in the assumption that $\int ds/\sqrt{y} = CV/\sqrt{EH}$. Also, the restriction of the curve to those of the same length meant that he found, not one curve, but an infinity, corresponding to given path lengths. All are catenaries, but there is no reason to say one is any faster than another.

There is some evidence that Gregory although convinced in his

own mind that he had found the brachistochrone, was not sure that his arguments were sufficient to convince the scientific world. Another paper of this set gives a list of, at times almost metaphysical, reasons for accepting the catenary as the curve of quickest descent. For example, the sine law of refraction may be deduced by the least time principle, as Fermat had done. Here, we have another least time calculation and it is fitting that the answer should be a curve (such as the catenary) which appears naturally, thus providing some sort of parallel with this sine law calculation. Some of his reasons are confused, others are not unique to the catenary; if the given points lie in a vertical line the catenary will be a vertical line, which coincides with the brachistochrone, but this would be true of other curves such as the parabola. The best geometric reason, Gregory felt, would be an analysis based on the forces acting on the parts of the brachistochrone, which might be shown to coincide with those acting on the catenary. Unsurprisingly, this train of thought led nowhere, but it underlines Gregory's discontent with the path which had led to his personal conviction that the brachistochrone was, in fact, the catenary. Nevertheless, by February, 1697, he was still quite satisfied that (although he still lacked a totally convincing proof) he had found the brachistochrone.

His attempt to use the continental methods by applying them to two infinitesimal increments of the curve had failed. Yet it was just this approach which Newton, and others used to solve the problem⁸⁷. However, they considered only uniform speed over the infinitesimal element, whereas Gregory had unnecessarily complicated

⁸⁷ E.g. de l'Hôpital, 1699 C123; Newton, 1700 C122.

his analysis by considering accelerated motion. One feels that this was a fault on the right side, as Gregory was more often prone to make unjustified simplifications. Had his analysis ended there, it might have remained a commendable attempt on a difficult problem.

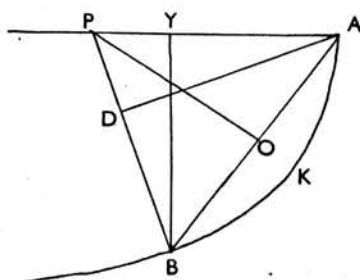
However, he did at last introduce the assumption of uniform motion and derived the expression $\delta t \propto \delta s / y^{\frac{1}{2}}$. Unfortunately, he leapt immediately to length of curve divided by the square root of the distance of the centre of gravity below the horizontal line through the higher point, and thence to the catenary. As long as Gregory had no specific answers in mind, his work was careful and (if we excuse his needless complications) valid. Nevertheless, once he had glimpsed the catenary on the horizon of possible solutions, he forgot all his previous care and went straight for this solution. He does not seem to have once returned to the expression $\delta t \propto \delta s / y^{\frac{1}{2}}$ to see if it would yield an alternative solution. Gregory's first attempt to show how he, too, could handle the new calculus methods was a failure, and he was to discover this from Newton.

As we have seen, in January, 1697 Newton received Bernoulli's challenge to find the brachistochrone, and on the 30th of that month he sent his solution to Montague for inclusion in the Transactions. On 23rd January, Gregory had entered in workbook E his conclusion that the catenary is the brachistochrone. Below this, however he noted that this was not so; in a letter to him dated 11th February (now unfortunately lost) Newton had told him that the required curve is a cycloid⁸⁸. Gregory turned at once to the problem of descent in a

⁸⁸E72.

cycloid, writing on 13th February the first draft of the paper which would appear anonymously in the Transactions⁸⁹. Later he returned to his 'proof' that the curve is a catenary only to note that this is not so, and that his calculations were all wrong.

The paper which Gregory now produced perhaps as a conscious development of the line of thought which led him to mention the cycloid in his search for the brachistochrone, was a relatively elementary development of Huygens' work on the cycloid. It seems though, that his first intention was to develop therefrom a proof



that the cycloid is the brachstochrone.

Let A and B be the two given points, and AKB a cycloid through them. PB is perpendicular to the curve at B, AD and PC are perpendicular to PB and AB, while YB is the

vertical through B. Gregory's theorem shows that time of descent through AB: time of descent through AKB is as $AB \times BP$: $AP \times BY$. In a corollary to this first draft he tries to argue that as BY represents the path of swiftest fall from the horizontal through A to the point B, so the cycloid (by virtue of the above proportions) will be the path of swiftest fall from A to B. However, he appears to have recognised the unpromising nature of this line of argument, as he dropped it from a later draft.

The later draft of this paper which lies among Gregory's manuscripts, in his hand and corrected by him, is just as the paper

⁸⁹ 13.2.1697 Alll.

which appeared in the Transactions⁹⁰. Now that he is no longer attempting to prove that descent is swiftest in a cycloid but is only comparing it with descent through a straight line, he can use a neater geometrical construction in his theorem. This refinement had originally been a corollary to the main theorem. The corollary described above is omitted, but otherwise the paper is essentially that which he wrote on receipt of Newton's letter telling him that the brachistochrone is the cycloid.

There can be no question but that this paper, published anonymously, was David Gregory's. We have his own first and final drafts, the latter indexed by him as 'A paper of my own about the descent in a Cycloid printed in the Transactions in 1697'⁹¹. The Royal Society papers have a fair copy in Gregory's hand, which has noted on the back 'Gregory de descensu gravium ... Read Mar.17.96' [i.e. 1697 in our dating]⁹². The paper was published in the Transactions for February, 1697⁹³.

I have found no reason why Gregory should have chosen to have the paper published anonymously. If he was ashamed of his failure to prove that the curve was the brachistochrone, surely a dignified silence was better. If he believed his paper made a genuine contribution to the problem of descent in a cycloid, why not proclaim his authorship? Perhaps, the decision to publish anonymously was not

⁹⁰ A22.

⁹¹ Beginning of quarto A; entry under A22.

⁹² RS Cl.P. iii (2) 1660-1740 Mechanics.

⁹³ PT 19 (February, 1697) no 225 424.

Gregory's but was taken for him by the Royal Society, though there is no obvious reason for such a decision. It may be that Gregory was pleased with his paper and wished to publish, but that Newton realised how far short it fell of answering the problem of the brachistochrone. In that case, Newton might well have been reluctant to allow the continental mathematicians to see that this was the best his disciple could produce. It may have been as a compromise between Gregory's desire to publish and such a reluctance of Newton's that the paper finally appeared anonymously.

Ironically if this last was the reason for anonymity, many authors have attributed the paper to Isaac Newton. Castiglione seems to have been the first to do so, when he edited Newton's works in 1744. After Newton's letter to Montague of 1697, he published this paper of Gregory's⁹⁴. However, he had warned the reader in his preface that the authorship of this piece was unknown. But, since it is brief and to the point and seems to savour of Newton's ingenuity, he will include it so that others may judge for themselves⁹⁵. Horsley's collection of Newton's work, which followed in 1782, contains no such caveat. Gregory's paper follows Newton's letter to Montague, answering Bernoulli's challenge, as if there were no doubt at all of its authorship⁹⁶. In 1809, the editors of the abridged Transactions were

⁹⁴ Isaaci Newtoni Equitis Aurati Opuscula Mathematica Philosophica et Philologica. Collegit partimque Latine vertit ac recensuit Joh. Castiglioneus 3 toms. (Lausanne and Geneva, 1744) 1 Item VI 285-92.

⁹⁵ Ibid 1 viii.

⁹⁶ Isaaci Newtoni opera quae exstant omnia commentariis illustrabat Samuel Horsley LLD RSS (London, 1779-85) 5 vols. iv (1782) 411-18.

also of Castiglione's opinion, and commented that

'This anonymous paper has very much the character of a production of Sir Isaac Newton'⁹⁷.

No doubt Gregory would have been delighted to know that his work might be taken for Newton's! It seems surprising now that it was, for, although the paper is perfectly competent, even elegant, it is far short of the proof Newton must then have possessed that the brachistochrone is the cycloid. Newton never did publish this proof, however, and perhaps his editors felt that Gregory's paper on the cycloid provided some sort of alternative in lieu of full proof.

This, of course, was just what it had been for Gregory. Unable to find the brachistochrone himself, or, knowing the result from Newton, unable to prove it, this paper was Gregory's best alternative. It is indicative, too, of his faith in Newton that, believing he had proven the curve to be a catenary, yet when he received Newton's statement (without proof, or his own attempts to prove it would not have been so wide of the mark) that the curve was a cycloid, he at once abandoned his earlier belief.

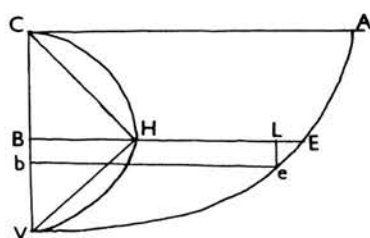
On 7th March, nearly two weeks after he had noted that his own calculations were wrong, Gregory met Newton in London and learnt something of his proof⁹⁸. Ten days later, Gregory's paper was read to the

⁹⁷ Abridged Philosophical Transactions edited by Charles Hutton, George Shaw and Richard Pearsons. IV 1694-1702, (London, 1809) 140n.

⁹⁸ London, 7.3.1697; A78.

Royal Society, and this meeting gave Newton an opportunity, if he so wished, to persuade Gregory to publish anonymously. The notes which Gregory took on this occasion are far from perfect, and I am indebted to Dr Whiteside for explaining the Newtonian analysis underlying Gregory's notes⁹⁹.

Newton's argument must have been something of this sort;



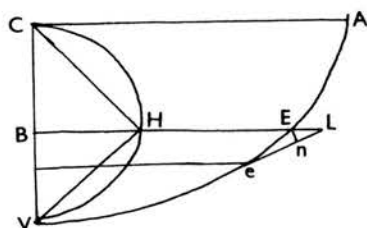
Given a trochoid, AEV,, with generator semi-circle AHV, we have velocity at E $\propto \sqrt{CB} \propto CH$.

Call EL, the increment of BE, O.

Since, in a trochoid, HV is parallel to eE, we have ΔeLE similar to ΔVHC , and so the increment of Ae, Ee , = $O \times \frac{CV}{CH}$. Thus, since CV is given, $\frac{O}{Ee} \propto CH$. Here Newton introduced a property of the required curve which he did not prove for Gregory (or, at least, Gregory did not copy down its proof, which is considerably more difficult than the above analysis);

in the brachistochrone, $\frac{O}{Ee} \times \frac{1}{\text{vel. in } Ee}$ is given.

But this is the property, $\frac{O}{Ee} \propto CH$, which we have found in the cycloid, q.e.d.



Gregory's account of this proof reveals his lack of understanding, as he attempted to reconstruct (perhaps from the memory only of his discussion with

Newton) the elements of Newton's proof into something which convinced him.

⁹⁹D.T. Whiteside letter to author: 8.8.1976.

He drew the figure as I have shown, again calling the increment of BE, EL, o. He seems to have considered the cycloid in comparison with another curve running closely alongside it.

Triangles CHV and EnL are similar, so that 'the increment of eE', or nL, ~~=o~~ $\propto \frac{CH}{CV}$, and so, since, he says, o and CV are given, nL is as CH. Therefore time through the increment of eE, $= nL \text{ (vel. thro' nL)} \propto \frac{CH}{CH}$ (since, as above, velocity at E is as CH), which is given. Thus, according to Gregory, the increment of the time through Ee is constant and so the time through all the Ee is a minimum (since it is not a maximum)!

Below this he points out that this analysis may be used to compare time through infinitesimal increments as curves lying alongside one another, joining the two given points. Thus the cycloid is shown to be a faster curve of descent than those on either side of it.

Of course, Gregory's argument is nonsensical - but the echo of Newton's analysis is there. It seems as if Gregory, not realising that the property of the brachistochrone had been introduced without proof, attempted to reproduce an entire proof from what Newton had explained to him. He made no attempt to set the proof out more formally, however, as he had with Newton's analysis of the solid of least resistance, say, so perhaps he had his misgivings about it! He did continue his interest in the curve, and there are further references to it in his papers.

On 20th February, 1698, Newton again mentioned the matter to him, telling him of his intention to propose a problem in turn to Leibniz and Bernoulli - that of the path of a projectile in a medium

Since time \propto dist/speed, and velocity in free fall is as the square root of altitude fallen through, $T_{DBC} = DB/\sqrt{QD} + BC/\sqrt{QF}$ and $T_{DSC} = DS/\sqrt{QD} + SC/\sqrt{QF}$. Sault equates these two, since they are both minima, to get

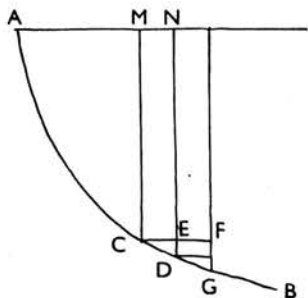
$$\frac{DB-DS}{\sqrt{(QD)}} = \frac{SC-BC}{\sqrt{(QF)}} \Rightarrow \frac{Br}{\sqrt{(QD)}} = \frac{tS}{\sqrt{(QF)}} . \quad \dots(1)$$

Now, we can say that triangles SBr and BtS are similar to triangles DSF and CHS (which is approximately so). Thus $BS/DS = Br/SF$ and $tS/HS = BS/CS \Rightarrow Br/CS \times SF = tS/HS \times DS$. Combining this with (1), $\sqrt{QD}/CS \times SF = \sqrt{QF}/HS \times DS$. But we can see that $DS = CS$, i.e. $\sqrt{QD}/SF = \sqrt{QF}/HS$. (That is, all our variables have hitherto been free. Now we choose s , the length of the curve, as our independent variable, and say δs is constant.)

Sault reduces this expression to a known fluxional equation for the cycloid, which is therefore the curve of quickest descent. (This analysis was somewhat confused in the Transactions by a number of misprints, but, although annoying these could not have prevented Sault's readers from following his proof.)

The weak points in this proof need no underlining; the arguments leading to equation (1) are scarcely convincing. However, Gregory saw clearly here for the first time the steps necessary in proving the identity of the brachistochrone. The primary analysis considers two adjacent portions of the curve and so introduces the crucial second order infinitesimals (Br and tS , here). A condition of the brachistochrone is so derived and then shown to hold in the cycloid.

Now Gregory could produce his own analysis, which is broadly similar to Sault's.



Let CD and DG be infinitesimal increments of the brachistochrone AB.

As before $T_{CDG} = CD/\sqrt{MC} + DG/\sqrt{ND}$. Call CF a, ED b, EF x and FG c.

Then $CE = a - x$ and

$$\frac{CD}{\sqrt{CM}} + \frac{DG}{\sqrt{ND}} = \frac{\sqrt{(a^2 - 2ax + x^2 + b^2)}}{\sqrt{CM}} + \frac{\sqrt{c^2 + x^2}}{\sqrt{ND}}. \quad (\text{In fact,}$$

$DG^2 = (FG - ED)^2 = x^2 + (c - b)^2$, while $c^2 + x^2 = EG^2$. However, this error is cancelled at (*) below.)

At a minimum, this expression will have zero fluxion, i.e.

$$\frac{-a\dot{x} + x\dot{x}}{\sqrt{CM} \times \sqrt{a^2 - 2ax + x^2 + b^2}} + \frac{x\dot{x}}{\sqrt{DN} \times \sqrt{c^2 + x^2}} = 0.$$

Whence

$$\frac{CE}{\sqrt{CM} \times CD} = \frac{EF}{\sqrt{DN} \times DG} \quad \dots (*)$$

Now let $CE = EF$ (make the variable expressing the abscissa, whose fluxion is x , the independent variable, as Sault did for s above).

Then $\sqrt{MC} : \sqrt{ND} = DG : CD$ which is the property of the cycloid above.
q.e.d.

The resemblance to Sault's paper is obvious. Gregory has found an alternative passage, from the basic conditions to the cycloid property, but the essentials are unchanged. Moreover, his passage employs Newtonian fluxions with which Gregory was most at home.

There is as Dr Whiteside has pointed out to me an even closer resemblance between this paper of Gregory's, and an analysis of John Craige's which appeared in the Transactions in January, 1701¹⁰³. The

¹⁰³ D.T. Whiteside, letter to author: 2.9.1976; see PT 22 (January, 1701) no 268 750-51.

two men had clearly been thinking along the same lines, but there is no reason to suppose either had seen the other's work. It is clear from the corresponding entry in the index to quarto A that Gregory's paper was written by the spring of 1700, and there is no suggestion among his manuscripts that he had seen Craige's paper before publication. After the publication of Sault's paper, this approach and fluxional analysis was an obvious solution to the problem.

In 1699, two more analyses of the curve had appeared. The Marquis de l'Hôpital, whose powers Newton doubted, had produced a solution of which Gregory procured a copy¹⁰⁴. Also, Fatio de Duillier had published his solution, which employed the radius of curvature property of the cycloid¹⁰⁵.

In early spring, 1700, Gregory was in London and procured Newton's lunar theory for publication in his Astronomiae¹⁰⁶. On 1st April, Newton presented him with a full solution of the brachistochrone problem; a solution which, as Dr Whiteside has pointed out to me, simplifies Fatio's radius of curvature analysis¹⁰⁷. Gregory apparently made no attempt to study this solution further; he was busy with his Astronomiae and even after its publication other cares arose for him.

¹⁰⁴ C123.

¹⁰⁵ Fatio de Duillier 'Lineae brevissimi descensus investigatio geometrica duplex' appended to Fruit walls improved ... (London, 1699).

¹⁰⁶ 28.2.1700, with notes of 25.3.1700 C121² RG fo 15.

¹⁰⁷ 1.4.1700 C122² RG fo 22.

In March, 1698, Jacques Cassini had visited Gregory at Oxford and told him of the attempts made on the continent to find the brachistochrone. De la Hire and Sauveur had both published their solutions in the Journal des Scavans, but they had claimed that it was, respectively, a cubical or an ordinary parabola¹⁰⁸. Gregory must have been relieved to hear this- other mathematicians had failed with this curve. At least his own failures had not been published!

In truth, it was a very difficult problem, and we must not judge Gregory's failure with it too harshly. Yet, his record in this matter is not bright. First he convinced himself by a chain of suspicious reasoning that the catenary was the required curve. Then, learning that the cycloid was the curve he sought, he attempted to prove that, but could only produce for the Transactions a relatively elementary work on descent in the cycloid. When Newton attempted to explain his derivation, Gregory could not understand him at all (though this says something of Newton's powers of explanation as well as Gregory's of comprehension!). Only after he saw Sault's solution, neatly printed in the Transactions where he could study it at leisure, could David produce his own proof that the brachistochrone was a cycloid.

Leibniz had suggested making a list of those mathematicians who could solve the problem¹⁰⁹ - this, presumably, not only identifying a group of 'super-mathematicians', but also contrasting the numbers

¹⁰⁸ A67 RG fo 73.

¹⁰⁹ Sault's article on brachistochrone¹⁰² 425.

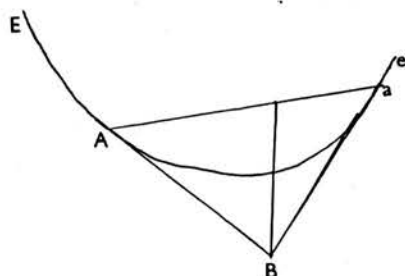
of successful Leibnizian disciples with Newton's. This, in itself points out the magnitude of the problem.

Yet Gregory's failure here must have made him even more determined to succeed in his next attempt; the catenary curve.

4.6 The Catenary Curve

The catenary is the curve which a uniformly weighted chain (catena in Latin) takes up when suspended at its ends but otherwise hanging freely under gravity. Today we would express its equation as $y/a = \frac{1}{2}(e^{x/a} + e^{-x/a})$ or $y/a = \cosh x/a$. In the late seventeenth century, however, this could be expressed only by referring to various other curves and a geometric construction therefrom. This curve has been discussed at length by Truesdell¹¹⁰.

Since the beginning of the seventeenth century this curve had been examined by various authors. Gregory believed that it was a parabola and Huygens had also looked at the problem. In 1673, Pardies published La Statique at Paris, which contained the basic principles on which the later work of Leibniz and John Bernoulli



would be based. The curve, Pardies claimed, would remain unchanged if any of its parts were solidified. In particular, it will be unchanged if we replace the parts of the chain above A and a by suitable forces acting

tangentially to the curve. His statistical principle stated that the vertical through the point of intersection of the tangents at a and A would pass through the centre of gravity of the portion aA. He had proven further that the catenary was not a parabola, but it was almost twenty years before any advance was to be made on this.

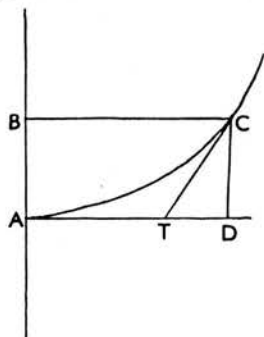
In the Acta for May, 1696, James Bernoulli proposed a contest

¹¹⁰ Truesdell op cit⁷⁶ 64-75.

to find this catenary curve, and in the following year three solutions were published¹¹¹. The solution of Christiaan Huygens was restricted to special cases and its only statement of principle appears to have been the erroneous $x/s = f(dx/dy)$ where s is the length of the chain. In a letter to de Beaval, published in the Histoire des Ouvrages in 1693, Huygens extended his treatment, but his geometrical methods were simply not sophisticated enough to handle this very difficult problem¹¹².

The more important solutions were those of Leibniz and of John Bernoulli, James' younger brother who first made his mark upon the scientific world with this paper. Both used the earlier work of Pardies to tackle the curve.

Leibniz used the fact that the intersection of two tangents lay on the vertical through the centre of gravity of the included portion of the curve. In a letter to Huygens he showed how this gave him the basic differential equations to the curve¹¹³.



Consider the portion AC of the curve, where A is the vertex and C any point on it. Let $AB = x$, $BC = y$, $AC = s$. Since T is on the vertical through the centre of gravity of AC,

- ¹¹¹ James Bernoulli AE (May, 1690) 217-19.
 Huygens AE (June, 1691) 281-82.
 Leibniz AE (June, 1691) 277-81.
 John Bernoulli AE (June, 1691) 274-76.

- ¹¹² Histoire des Ouvrages des Scavans (December - February, 1692-93) 244-57.

- ¹¹³ Leibniz to Huygens: 14.9.1694 Huygens op cit⁸ X, 679.

$AT = \int y ds/s$. But CT is the tangent at C , and so AT also = $y - x dy/dx$.

If y is the independent variable, and so dy is constant and $d(dy) = 0$, he derives the basic principle $a/s = dy/dx$. From this we find $dy = adz/\sqrt{z^2 - a^2}$, where z is $x + b$, b a constant of integration. This leads him to $x/a = \frac{1}{2}(e^{y/a} + e^{-y/a})$.

The paper which he published in the Acta begins with the geometrical construction of this equation. It is followed by corollaries giving constructions for tangent, area and length of curve, and the centre of gravity of any portion of it. The principle $a/s = dy/dx$ is immediately derivable from the construction for tangent and for length, but it is not explicitly stated. All these constructions are given without proof, and the underlying differential equations are not mentioned. In particular, the basic importance of the position of the centre of gravity of a portion of the curve is not mentioned.

The derivation of John Bernoulli's work is given in his Mathematical Lectures, unpublished until 1742¹¹⁴. From Pardies' principles, he set down 5 axioms for the tension in a hanging cord and was thus able to derive directly the principle $a/s = dy/dx$. He does not seem to have discovered Leibniz's form $x/a = \frac{1}{2}(e^{y/a} + e^{-y/a})$, but his geometrical constructions are analytically equivalent to it.

These are given in the Acta; like Leibniz's, they are without proof or justification. The first derives the catenary from the integration of an equilateral hyperbola, and the second from the rectification of a parabola. In each case we derive the differential

¹¹⁴ John Bernoulli's 'Lectiones mathematicae ...' Opera Omnia 3 (Lausanne and Geneva, 1742) 385-558.

equation $dy = adx/\sqrt{x^2+2ax}$, which is the same as that given to Huygens by Leibniz, except for a change in origin.

The first property of the curve which he gives is a geometrical statement of the principle $a/s = dy/dx$. Other properties follow, including the length of the curve, the area under it, properties of its evolute and the centre of gravity of a portion symmetrical about the vertical axis. Both Leibniz and Bernoulli state a version of the extremal principle, used widely by Huygens, that the centre of gravity of the curve descends as far as possible.

These constructions and properties were all given without proof. Moreover, Leibniz had stated that his differential calculus was the key to the solution. As we have seen, David Gregory was, by the end of 1696, well-versed in this method. (Although to him it was the method of fluxions rather than of the differential calculus.) He had also made a close study of the particular problems solved by the continental mathematicians and of the basic principles they employed to reach a mathematical formulation. Now he felt he was ready to produce comparable work of his own, and the catenary (along with the brachistochrone) seemed a suitable problem.

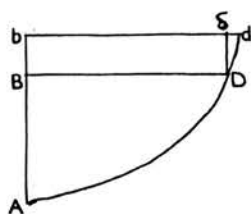
As early as 1693 he had discussed these curves with Fatio and Huygens. The former had mentioned that this curve is, when inverted, the true form of the arch, and the latter had disagreed with James Bernoulli's contentions that a sail inflated by the wind would be partly circular and partly a catenary¹¹⁵. The preliminary notes for Gregory's paper on the curve are partially dated January, 1697 and

¹¹⁵ London, 31.3.1693 A37; Hoffwyck 6.6.1693 A4.

the draft of the paper which actually appeared in the Transactions was written on 23rd January, 1697¹¹⁶. We can assume then that the major work on the curve was done in early winter, 1696-97, at much the same time as the work on the brachistochrone.

At the beginning of the paper, Gregory sets out his intentions. The important problem of the catenary curve has been studied by Huygens, Leibniz and Bernoulli, who have given constructions for it, but without proof. Now, by using Newton's method of fluxions, Gregory will prove these things and also add some new properties of his own.

The crucial first proposition of Gregory's paper attempts to derive the principle $s/a = dx/dy$ from statical considerations. Unfortunately, the preliminary notes are fragmentary, and we cannot tell how Gregory realised the importance of this relationship. However, it is in the papers of both Leibniz and Bernoulli (implicitly in the former, at any rate) and it should not have been too difficult for Gregory to pick it out. More importantly, we have no preliminary notes on the erroneous argument by which he derived it.

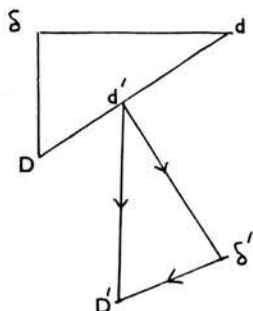


If AD is one half the catenary, vertex A, Gregory considered the relationship between $d\delta$ and δD , the fluxion of the abscissa and of the ordinate, or if $AB = x$, $BD = y$, between \dot{y} and \dot{x} . He wished to show $\dot{x}/\dot{y} = s/a$, or $D\delta/d\delta = AD/a$.

First, he says that, if Dd represents the weight of Dd , then $d\delta$ will represent that part of it which acts normally to Dd , and

¹¹⁶ Cl19, Al.

so pulls Dd towards the vertical position (we consider d as fixed). This is immediately clear if we consider the vector triangle $D'\delta'd$, congruent to $D\delta d$. If $d'D'$ represents the weight of dD , concentrated



at the mid-point of dD , we have

$$\overrightarrow{d'D'} = \overrightarrow{d'\delta'} + \overrightarrow{\delta'D'}.$$

Then, claims Gregory, we may take $d\delta$ as constant (taking y as the independent variable), in which

case this normal component of the weight

of Dd will also be constant, call it a . However, a is a line of finite length and we cannot, as Leibniz points out, use its ratio to an infinitesimal (Dd) to represent the ratio between two infinitesimals, the normal component of the weight of Dd and its total weight. Moreover, a would, if taken in this way, depend on the arbitrary way in which we divided the abscissa and would not be a constant dependent only on the whole catenary curve.

The next step is somewhat confusing. As is clear from the vector triangle, we can represent the weight of Dd acting along Dd by the line $D\delta$. However, Gregory seems to take this line to represent the tension at D acting in the direction Dd , balanced by the weight of the chain AD , which is as AD itself since the chain is uniformly heavy. Again he is representing by a finite line an infinitesimal force and this balances the previous step, when we say $d\delta/a = D\delta/DA$, or, in the notation used above, $\dot{y}/\dot{x} = a/s$. Thus his initial error lies in his incorrect balance of the gravitational forces on Dd , but, because he knew the required answer in advance, he was able to take an (erroneous) value for the tension which compensated for the previous error and gave the required answer.

However, in defense of Gregory, the difficulties of the approach he attempted, by considering the forces on an infinitesimal element, must be emphasised. Leibniz and Bernoulli had both used Pardies' earlier work, of which Gregory seems to have been unaware. Moreover, both the continental mathematicians had considered, not an infinitesimal but a finite portion of the curve. Almost certainly, Gregory could not have duplicated Leibniz's confident handling of integration and differentiation, far less Bernoulli's mechanical insights, to have derived the basic equations as the continental mathematicians did. Nevertheless, it is worth noting that when Leibniz heavily criticised Gregory's analysis, he gave no alternative discussion of the forces on an infinitesimal element.

As Truesdell points out, Gregory's major error lay in his failure to appreciate that it is the difference between the tensions, on each end of the element which balances its gravity. He continues to say that

'[Gregory's work] is one more example to show that the local balance of forces which we are all taught to regard as the simplest approach to the mechanics of continuous media, is in fact not an obvious concept'¹¹⁷

After this first proposition, Gregory's analysis continues smoothly. Propositions 2 to 6 derive the results of John Bernoulli's paper. 2 and 3 show that his two constructions of the curve give rise to $\dot{y} = a/\sqrt{2ax+x^2}\dot{x}$, and thence, with $s = \sqrt{2ax+x^2}$, we have

¹¹⁷ Truesdell op cit⁷⁶ 86.

$\dot{y}:\dot{x} = a:s$, as required by proposition 1. The following three propositions derive in turn Bernoulli's construction of the area under the curve, the position of the centre of gravity of a portion symmetrical about the vertical axis, and the nature of the curve by whose evolution the catenary is described. These and their corollaries prove all Bernoulli's results neatly and competently. Gregory also follows Bernoulli and Leibniz in stating the variational principle, that the catenary has the lowest centre of gravity of all curves of the same length, but, like them, he can offer no real proof of the principle. He calls this corollary 2 to proposition 5 in which he had found the centre of gravity of the catenary, but he does not derive it thence. It is instead an obvious extension of the axiom that the centre of gravity of any real system will descend as far as possible. He claims that all other properties may be deduced from this principle, but there is no sign that he attempted to do so.

Two results in particular are found here, which were not in the papers of Leibniz and Bernoulli. He states (corollary 7 to proposition 2) that the catenary is the form taken by a sail inflated by a uniform wind blowing parallel to a given direction. Here we equate the force of the wind with the similar force of gravity on a hanging chain and the result follows. This problem had formed one of the topics of conversation between Huygens and Gregory in 1693.

The other result was that the inverted catenary is the true form of the arch, that is, the only such figure which is self-supporting under gravity. Physical arches stand only because they contain the catenary within their structure. This had been known to Robert Hooke, who published the result in 1676, but in the form of

an anagram¹¹⁸. Gregory apparently first met the concept in March, 1693, when it was mentioned in discussion with Fatio¹¹⁹. Later, Newton was to prove this property for him, but unfortunately the scrap of paper on which he did so is undated¹²⁰. All we have of this proof of Newton's is a rough sketch, headed by Gregory 'Newtoni Demonstratio Catenarian erectam esse arcum fortissimum', and so we cannot tell if Gregory's justification of this claim, based on a discussion of the forces involved in the erect and dependent catenaries, was derived from Newton's proof or not. However, we can be certain that the idea was first put to Gregory by Fatio de Duillier, and was not his own. Indeed, in his reply to Leibniz's criticisms, Gregory freely admitted that this property was an already well-known one.

The seventh and last proposition with its ten corollaries proves the results of Leibniz's paper. The major result, $\frac{x+a}{-a} = \frac{1}{2}(e^{y/a} + e^{-y/a})$, is shown to give the catenary curve by showing that this gives rise to the differential equation $\dot{y} = ax/\sqrt{2ax+x^2}$ which has been already shown to define the catenary. The properties stated by Leibniz are all derived hence with little difficulty. The only exception is the discovery of the centre of gravity of a portion of the curve which is not symmetrical about the vertical axis, the property, of course, which had been basic to Leibniz's analysis of the curve. The problem of deriving the equation

¹¹⁸ Robert Hooke A description of helioscopes and some other instruments (London, 1676) ad fin.

¹¹⁹ A37.

¹²⁰ C57.

$\int y ds/s = y - x dy/dx$ from $s/a = dx/dy$ is far more difficult than that of finding the other properties of the curve, and this difficulty is reflected in Gregory's discussion, which is unconvincing, at best!

However, although it is easy to find fault with Gregory's handling of basic mechanics, the mathematics in this paper, with the exception of this last derivation of the centre of gravity, is quite competent. Once again, though, Gregory is deriving results previously found by others, and adds little of his own.

The paper appeared first in the Philosophical Transactions for August, 1697 and then in the Leipzig Acta for July, 1698. It also appeared in 1697 as a separate pamphlet¹²¹. Gregory was proud of the paper, and when his portrait was to be painted he considered whether he should include the figure of the catenary in the background¹²².

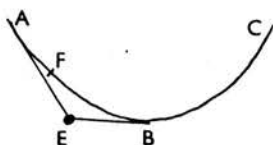
In February, 1698, Gregory discussed the problem of weighted lines with Newton who had, reports Gregory, then solved the dual problem of determining the figure of a weighted curve and of determining the weights necessary to produce a given curve¹²³. The solution was based on the fact that the horizontal tensions at all points on a catenary are the same. Gregory does not give us Newton's proof of this property, but it follows from the principles used by Bernoulli in his examination of the curve.

First, the forces acting at A and B are those which must

¹²¹ PT XIV (August, 1697) no 231, 637-52; AE (July, 1698) 305-21. A copy of the pamphlet, D. Gregory Catenaria (Oxford, 1697) is in Bod. Savile Cc 2.2.

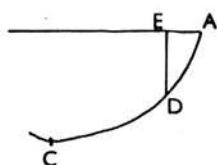
¹²² El50. The portrait is unknown today.

¹²³ 20 & 23.2.1698, A90.



act tangentially to support a weight E equal to that of the chain AB . In particular, this holds if B is the lowest point and EB is horizontal. Now, if the chain is suspended from any intermediate point F , it retains its original figure between F and C and the forces acting on all points of it are the same. In particular, the force at B is unchanged. Call this force ka , where ks is the weight of the chain AB , equal to the weight E . By considering the forces acting on A , we derive immediately $s/a = dx/dy$. But it is also clear from this analysis that, whatever E , the horizontal component of the tension at A is $-ka$; that is, the horizontal component of the tension is the same at every point of the catenary, and this is the result used by Newton. It was also this result which Gregory seems to have attempted to display in the first part of the proof of his first proposition.

In an undated passage in workbook E, Gregory used this principle to establish $s/a = dx/dy$ ¹²⁴. This was almost certainly written after these talks with Newton. Here he says simply that the horizontal



force on A is constant for any point A on the catenary 'because of the same tension through all of its parts'. The downward force (or vertical component of the tension) is as the weight of the chain AC , that is, as AC itself. But these forces are as AE and ED , so we have $AE:ED = a$ constant line: AC . His statements about the components of the tension at A are almost entirely unsub-

¹²⁴ E95.

stantiated, and it is doubtful whether Gregory could have substantiated them. However, they are certainly far easier to justify than the steps of his original analysis! As in the case of the brachistochrone, once a suitable approach was indicated, Gregory was able to follow it up.

Meanwhile, his errors had not gone unnoticed on the continent; both Leibniz and Bernoulli were swift to point them out. Leibniz's anonymous paper appeared in the Acta in February, 1699¹²⁵. His criticisms centred on Gregory's first proposition, and especially on the loose way in which he had used ratios between infinitesimal and finite quantities. The comments were telling ones, and perfectly justified. He ended with a challenge to Gregory to refer the question to Newton's judgement if unconvinced by these arguments.

It seems most unlikely that Gregory took up this challenge, for in his reply to Leibniz he retracts none of his earlier statements¹²⁶. Some years later when Gregory was forced to publish a paper in the Transactions retracting the claims he had made in his Astronomiae about Cassini's orbit, Newton had persuaded him to make his retraction as inconspicuous as possible. Indeed the paper was presented as Gregory's further thoughts on the orbit and the retraction consists of one, easily missed, phrase, in the middle of the paper (see 5.3.1). If Newton was so reluctant to have Gregory publicly admit to an error pointed out privately by Halley and de Moivre, how much more reluctant would he have been that Gregory admit to one pointed out in the

¹²⁵ AE (February, 1699) 87-91.

¹²⁶ PT XXI (December, 1699) no 259, 419-26.

Leipzig Acta? Had Newton entered the matter, anonymously, or even simply in private conversation with Gregory, he would have been forced to agree with Leibniz's criticisms and would thus have precipitated the very confession of error in his avowed disciple which he wished to avoid. He had already indicated a valid derivation to Gregory, and it seems that he went no further. Even if Gregory had appealed to him, Newton could only have refused to become involved.

Gregory was, of course, unable to refute Leibniz's criticisms adequately. He justified his action in publishing proofs of previously known results, and then merely elaborated on his original arguments. He skated around Leibniz's major criticism by claiming that he had not used the finite Δ to represent the normal component of the weight of his infinitesimal portion, but to represent the action of this component - an argument which justifies nothing.

However, Gregory's faith in his paper seems to have remained unshaken, and in May, 1701, he was still prepared to go into battle on its behalf. He resolved then to find out the identity of the anonymous author who had criticized him (he never seems to have discovered this - perhaps his confidence would have been shaken had he realised that the criticisms were those of Leibniz himself!) He also decided to discover whether an answer had been given to his answer, presumably so that he might again reply¹²⁷. Leibniz apparently felt, though, that he had made his point, and it needed no further elaboration, for he never made such an answer.

James Bernoulli had also had his comments to make on Gregory's

catenary curve. The seventh 'Epimetra' at the end of the fourth part of his 'De Seriebus Infinitis' read, in Truesdell's translation,

'David Gregory's analysis of the catenary curve, recently published in the Leipzig Acts for July, shows neatly how it is possible for us to be misled through an inevident and false though plausible argument to a true conclusion'¹²⁸,

Gregory noted these remarks in workbook E on 7th July, 1701. He decided that an opportunity must be taken by Keill or some other to say that Bernoulli's system of comets and his work on the gravity of the ether are not only inevident and false, but not even plausible to anyone but the author himself¹²⁹. Gregory, too, could be vengeful, and, like Newton, he preferred that another fight his battles for him. It is interesting that Keill, later to become Newton's 'war-horse' in the Leibniz controversy, was Gregory's first choice as champion.

As late as 1704, Gregory still evidenced his faith in his analysis of the catenary curve. In this year he planned a comprehensive text on the calculus, whose final book would consist of examples of its application (see 4.7). Along with the elastica and other curves from the Acta he intended to discuss the catenary here. This work was never completed, but there can be no doubt that, if it had been,

¹²⁸ James Bernoulli Positionum de seriebus infinitis ... pars quarta (Basel, 1698) 'Epimetra 7', quoted by Truesdell op cit⁷⁶ 85 n3.

¹²⁹ E96.

the mathematics would have been competent, but let down by a lack of understanding of basic mechanics - just as had happened when Gregory attempted to analyse the catenary curve.

4.7 After the catenary and brachistochrone

Gregory's work on these curves in the winter of 1696-97 marked the virtual end of his studies of the new calculus. He continued in workbook E for several months and some isolated examples arose out of his Astronomiae, but he no longer studied attentively the articles in the Acta, nor strove to supply them with proofs and elaborations. The examples he had worked over in his workbook E were never used to extend the 'Tract on fluxions'.

However, this seems to have been because new interests were absorbing his time, rather than because of any feeling of failure. Prodded by Newton, perhaps in a deliberate attempt to divert his efforts, he turned first to astronomy and then to ancient geometry. The Astronomiae was begun in April, 1697 and appeared in print in 1702. Gregory's edition of Euclid followed in the next year, and thereafter his preparation for the edition of Apollonius occupied his time. In the final years of his life he was also very busy with the work for the Scottish Mint. He left only his draft plan of a grand work on calculus, drawn up in 1704, to show that he had not renounced all claims to expertise in this field.

In 1697, though, when the work on the Astronomiae had only just begun, he spent some time studying the letters which had passed between Newton and Leibniz in the 1670's. This was not undertaken as a study in mathematics, though, but rather as one in priority.

In March, 1697, Gregory was in London talking to Newton, discussing, among other things, the problem of the brachistochrone. He also noted

'Newton's *Commercium Epistolicum* with Leibniz is to be published, with added examples taken from a book cited therein, and the letter of Sluse deriving the latter method from the former'¹³⁰.

This publication was to be in the forthcoming 3rd volume of Wallis' *Opera*¹³¹ and Gregory devoted much time to helping collect and prepare these letters for publication. St Andrews University Library contains the copies of these letters which Wallis and Gregory collected¹³². They make up a virtually complete record of the letters which passed between Newton and Leibniz, and Gregory studied them carefully and made summaries of their contents with notes as to their probable order. By 14th September, he was familiar with at least the most important letters. He concluded thence that Newton's *Epistola Posterior* was crucial in providing Leibniz with an understanding of the calculus. The German's letters of August and November, 1676, showed no knowledge of the process, yet that written on 21st June, 1677, after he had received Newton's letter, at least showed some evidence of these ideas. It was this letter of Newton's which contained the two (totally incomprehensible) anagrams, purporting to give the method of fluxions.

Wallis wrote to Gregory on 22nd January, 1698, discussing the letters and asking Gregory to inspect the Royal Society records for

¹³⁰ A78.

¹³¹ John Wallis *Opera Mathematica* 3 (Oxford, 1699) 'Collectio Epistolarum' 617-52.

¹³² SUL Gregory vol. 2 MS 31,010.

any relevant correspondence from Tschirnhaus¹³³. Newton himself was interested in the success of this publication, for he sent Wallis four letters for inclusion, availing himself of the journey James Gregorie (probably David's brother) was making from Cambridge to Oxford in September, 1697¹³⁴.

Newton had first told Gregory of this publication. Whether his help had been enlisted then or by Wallis, Gregory clearly felt he was helping Newton in this task. We know today, beyond any doubt, that Leibniz had, as he claimed, deduced his calculus by studying the work of Pascal¹³⁵. However, although Gregory's notes never say so outright, they make clear his belief that Newton's Epistola Posterior had been the crucial impetus. Fatio's thinly veiled accusations of 1699 may have been triggered off by an imagined slight he had received from Leibniz¹³⁶. Neither Gregory nor Wallis was rash enough to state publicly that Leibniz not only was the second inventor of the calculus, but also that his 'invention' was probably taken from Newton's work. However, the notes on these letters made it clear that David Gregory (and presumably also Wallis, for they worked together on the letters) privately agreed with Fatio's comments. Unfortunately, it is now impossible to gauge how great a part Newton played in influencing them to this opinion, but it is clear that whatever prompted him to publish them at that moment, Fatio was not alone in his views.

¹³³ Ibid fo 115.

¹³⁴ Ibid fo 126.

¹³⁵ J.E. Hofmann Leibniz in Paris, 1672-76 (London, 1974).

¹³⁶ Fatio de Duillier, op cit¹⁰⁵

Apart from the occasional example in the Astronomiae, examined in the next chapter, Gregory did not turn again to the calculus until January, 1704¹³⁷. At this time both his Astronomiae and the edition of Euclid had appeared, and neither the work on Apollonius nor state business were as yet very pressing. Gregory therefore had leisure to contemplate a text book on calculus.

The work to be entitled 'Contactus et Tetragonismus' was first intended to be divided into seven books, the first dealing with the work of the Ancients. It would consider the curves they had constructed and the areas they had measured before the 'restored knowledge' of the sixteenth and seventeenth centuries. However, Gregory decided later to restrict this book to a preface, so that, like the Astronomiae, the 'Contactus' would consist of a preface on the work of the Ancients, followed by six books.

These six books would set out the modern knowledge of the differential calculus, along with its application in solving problems of physics and geometry. Gregory found that much of this work, especially among the modern authors, had been set down most obscurely, either through the author's style, or through his desire to conceal his methods of proof. The 'Contactus' was to remedy these ills by explaining and proving such work. The connection with the 'Tract on fluxions' and workbook E is obvious: the examples from these sources were directed towards precisely these ends.

The books were divided chronologically, from the indivisibles of the sixteenth century to the fluxions of Newton. First, then,

¹³⁷ Oxford, 29.11.1704.

came the work of Cavalieri, Torricelli and Gregory of St Vincent. To these, Gregory would add the similar work of Viviani on maxima and minima, and his own on Hippocrates' lunula and Viviani's problem of the fenestrated dome. Galileo's application of geometry to physics, 'the beginning of physical truth' and its application by Alhazen and Vitellio to optics would be included here.

Descartes and Fermat were the basis of the next book. The former's method of tangents was the first to promise universality, and the latter's was the basis for all later methods. Descartes' use of geometry in analysing the rainbow and Fermat's introduction of the least time principle into dioptrics served to illustrate the practical applications of these methods. Wallis' Arithmetica Infinitorum was also to be included here, and Gregory would show 'how great an addition to geometry is made here'.

The work of Sluse and Barrow followed in the next book, with the way in which their methods of tangents were built into a rule from which the method of fluxions was derived. The physical applications were found in the work of Barrow, who had approximated optical surfaces by equicurved spheres (which Gregory claimed to have reduced to a canon in his Optics). Huygens' Horologium Oscillatorium and Wallis' geometrical treatment of everyday mechanical principles were also examples of the applications of these methods.

The next book dealt with the work of James Gregorie. His Vera Quadratura examined figures by means of inscribed and circumscribed polygons, thus giving rise to convergent series. David criticises the modern use of convergent applied to series whose sum is the required limit, preferring that it be retained for such situa-

tions as his uncle had examined, where a double sequence (in the modern sense) converges to a limit. However, not all figures can be measured accurately, and such infinite sum series are very useful. Mercator first published such a series, whose derivation was proved by James Gregory. David refined the method, and published it with many examples in his Exercitatio Geometrica. John Craige and Leibniz did some similar work! Later, David Gregory published a further improved method of quadrature (the 'second method') but in the tract added to his Optics. Newton has gone further yet. (As the Optics, with the appended 'De Quadratura' did not appear until after this draft was written, we have further evidence here of Gregory's knowledge of Newton's plans.)

Newton's method of fluxions is the subject of the next book. The two problems of finding tangents and areas have now been resolved into the question of finding the equation involving fluxions from that involving fluents, and its inverse. The continental geometers have changed the terminology of fluxions and fluents into that of differences and differentials. They consider these quantities as already in existence, whereas Newton sees them as generated by motion. (Thus, although he did not display it in the 1694 'Tract on fluxions', Gregory was, by 1704, aware of this fundamental difference between the Leibnizian and Newtonian concepts. However, these words alone need not imply a very deep awareness - he may have been merely parroting a remark of Newton's. We cannot tell whether he would now have been able to display these differences when he discussed the fundamental concepts in greater detail.) Fluxions, or differences, of a higher order were also to be discussed in this book.

The final book would contain the application of these concepts to geometry and to physics. Such problems as the solid of least resistance, the elastica, the catenary and the brachistochrone would be discussed here. He would examine the physical meaning of higher order fluxions and the genesis of physically produced curves, such as causticae. In sum, he intended to expound all the unproven assertions and obscure results produced by the geometers who worked with fluxions and differences.

He hoped to devote four years to preparing this work and to have it ready for the press by June, 1709. In fact, he died over six months before this date. Little more was ever prepared in any case. He added a few more notes to this outline, on, for example, his intention, to split each book into sections as he had done with his Astronomiae, and the whole draft was copied into workbook E¹³⁸. No more traces of the work can be found among his papers.

It was an extremely ambitious plan. Clearly Gregory's failure with the catenary and the brachistochrone had not daunted his confidence - indeed these two particular curves were to be discussed in the final book. Had the work been published, it must have been an influential work, if only because it introduced the continental results to a far wider audience than read the Acta. The quarrel between Newton and Leibniz which was brewing as Gregory wrote this outline was to divide the British and continental schools throughout the following century. Perhaps Gregory's work would have helped to show the British that the continental geometers had something to

¹³⁸ E115-21.

offer.

Of course, it may have been for this very reason that the work was never completed. If Gregory consulted Newton about the plan, as he did over the Astronomiae, Newton would have been unlikely to have approved it as it stood. The methods of the calculus which were a climax to the work were clearly Newton's, but the book continued from there. The applications of the methods which followed (with the exception of the solid of least resistance and the brachistochrone) were taken from the continental geometers. The catenary, the elastica, the velaria, the paracentrica; all these had been solved on the continent by Leibniz's methods. Gregory's synopsis was consciously intended to show the power of Newton's method, but it also served to underline an unpalatable truth- whatever the theoretical advantages of the two methods, it was Leibniz's which was being used to solve these difficult problems, and Leibniz's whose power was being publicly demonstrated.

Truesdell comments that 'unlike Leibniz, Newton had no Bernoullis'¹³⁹. That is, at that time none of Newton's disciples were apparently able to apply his methods with the power he could himself. It was only after a new generation of British mathematicians grew up that this situation was to alter. Newton was already jealous of his priority of invention and may have had doubts of Leibniz's independent derivation of the calculus. He would not have wished this disparity of applications to appear so clearly.

Moreover, had he been consulted, Newton would certainly have

¹³⁹ Truesdell op cit⁷⁶ 86n.

doubted Gregory's capabilities for the task. As we have seen, Gregory had erred publicly with the catenary, derived the brachistochrone only after a completely false start and help from Sault's publication, and, even when Newton had spoon-fed the analysis to him, he was unable to derive the solid of least resistance correctly. If Gregory were to publish on such subjects, Newton would have had to check every word he wrote in order to save his disciple from further public error.

However, there is no evidence that Newton was consulted. Gregory became progressively more involved in his edition of Apollonius, and his work for the Mint, and these alone may stand as sufficient reason for his failure to complete this comprehensive text-book on the calculus.

Chapter 5

Astronomiae Elementa; 'opus cum sole et luna duraturum'¹

Gregory began to plan this work, generally accounted his masterpiece, in April, 1697 and it was published in 1702. In this period he produced the first astronomy textbook set in a framework of the physical system Newton had set out in his Principia.

The impetus to this work probably came with the realization that his Notae, the detailed commentary on the Principia which he had prepared, would not after all be published. Instead, he produced this work. Newton discussed it with him at several meetings over this period and Keill, Arbuthnot and probably Halley read it in manuscript and added their suggestions. The Astronomiae was in this way a joint effort by the early Newtonian scientists to produce a popular work on their philosophy.

Gregory's preliminary notes for the work indicate a wide range of source books; far wider in astronomy than was deemed necessary for a study of the Principia itself. Some of the authors he mentions used all Kepler's laws, though others preferred an approximating device to the second law. Almost all endorsed the Copernican system.

The shape of the work underwent many changes, but it was eventually organized into six books. The preface, supplied by Newton, considered the astronomy of the Ancients. Book one outlined Copernican astronomy as Newton had displayed it and book two gave

¹'A work to endure as long as sun and moon' John Keill Introductio ad veram physicam, seu lectiones physicae ... (Oxoniae, 1702) Preface.

the apparent, Ptolemaic system. Books three and four discussed the motions of the planets and their satellites, book five comets and book six comparative astronomy.

I have selected topics from these three chapters which seem ~~to~~ ^{to} best display Gregory's attitudes, the influences upon him and the way in which they acted. They are often also those to which he devoted most consideration. In book one he discussed three alternatives to the Newtonian cosmology; those of Kepler, Descartes and Leibniz. He was reluctant to criticize Kepler and treated Leibniz' hypothesis fairly, but found nothing complimentary to say about Descartes! Book two displays, in the treatment of observationally determined values, the features which distinguished Gregory's approach from that of John Flamsteed. Here I have also looked at the problem of atmospheric refraction on which Gregory expended considerable time and effort. His own attempt to build an a priori model of atmospheric refraction was unpracticable and, in spite of his earlier intention to do so, he included no tables of refraction in the Astronomiae.

Book three raises an interesting question in Gregory's handling of the physically accurate second law of Kepler and the mathematically simpler devices of Ward and Bouilleau. I have traced his attitudes to this problem from his days at Edinburgh and suggested how Newton's work influenced him here. With Cassini's orbit, he hoped to find a compromise between the two views, but his analysis of the orbit was faulty and he was forced to an embarrassing retraction of his statements about it. This book also contains a discussion of the observations Flamsteed had made which he believed were due to stellar parallax.

Book four contained Newton's lunar theory, and the publication of this theory further angered Flamsteed to whom it had previously been promised. Gregory's comments since 1693 on the lunar observations Flamsteed had made indicate his own changing attitude to the Astronomer Royal. Book five on comets introduces several theories, such as James Bernoulli's, which might not have been familiar to Gregory's readers, while book six extends the comparative approach taken in his Edinburgh astronomy lectures.

The preface and the lunar theory are well-known to be Newton's, but his advice, help and influence can also be detected in almost every topic. Gregory's personal loyalties and enmities seem to influence his scientific judgement at times. Perhaps this happened with his treatment of Cassini's planetary orbit: certainly it did with his discussion of Flamsteed's measurements of the supposed parallax effect.

Two interesting pieces of mathematics arise out of this study. The first, wherein Gregory tried to find a curve describing atmospheric refraction, was not published. The second example was his faulty analysis of Cassini's orbit. In each of these cases, it seems, Gregory had expected the results he believed he established to be true. He was therefore too uncritical of the methods he used to establish them and found himself in error.

The work enjoyed a period of fame and acclaim. Although in some ways it was eclipsed by works such as Keill's and Whiston's, aimed at an audience with less mathematical sophistication, it nevertheless maintained its reputation throughout the eighteenth century at least. It might not outlast the sun and moon as Keill had

hoped, but it lasted long enough to establish Gregory's reputation as an eminent Newtonian scientist.

5.1 Writing the Astronomiae

In this section I want to examine first the reasons why Gregory set about this work at this time. Then I will examine the sources he used, and the way the outline of the book developed, to include at last several pieces of Newton's.

5.1.1 Why the Astronomiae?

In the summer of 1694, Gregory had proposed to Newton that his Notae be published, but this design was tied up with proposals for a second edition of the Principia². As Professor Cohen has pointed out, though, Newton showed no interest in these notes or their publication³. Gregory noted on 4th March, 1696,

'I am not to revise my Notes on Mr Newton's philosophy, until there is a second edition of it by Mr Newton's self, or that we despair of having one'⁴.

Certainly by March, 1700, when Newton allowed Gregory to take a copy of his lunar theory, originally intended for a new edition of the Principia, but now to be published in the Astronomiae, it was clear that no second edition was planned for the near future⁵. However, in 1696 Newton moved from Cambridge to London, and became deeply involved in running the Mint. It is likely that by the time Gregory

² Cl2 partially in NCIII 461 384-6.

³ I.B. Cohen Introduction to Newton's Principia (Cambridge, 1971) 194-95, 199.

⁴ El. Hiscock 4.

⁵ See, for example, Newton to Flamsteed: 1.11.1694, NCIV 478 42-3.

began to plan his Astronomiae in April, 1697, he already realized that the plan for a second edition interleaved with his notes was unlikely to materialize⁶. The Notae might have been published alone, but there is no evidence that Gregory considered this possibility. Instead he began the Astronomiae, which took him five years to write, leaving unpublished his nearly complete Notae.

In the face of Gregory's clearly expressed wish of 1694, this cannot be readily explained simply as a loss of interest in the Notae. It is more profitable to consider how Newton might have felt about their publication. Gregory noted in 1694 that, if the Notae were published with a second edition, he would omit from them

'a great deal that serves to detect slips or even mistakes of Newton'⁷.

These would not have been omitted from a version intended to supplement the first edition, and it is easy to see why a publication of his 'slips or even mistakes' did not appeal to Newton! Even without these corrigenda, the implication that the Principia was incomprehensible as it stood, and in need of a step-by-step guide, was inescapable in the publication of Gregory's Notae. However justified this implication may have been, Newton could not have liked it, and may have felt (or persuaded Gregory) that it would hinder, rather than help, the acceptance of Newtonian science.

However, a work which expounded the applications of Newtonian

⁶ A60.

⁷ C42 in Turnbull's translation NCIII 386.

science to astronomy, a work suitable for use as a University textbook and with an eye to the needs of the practical astronomer would clearly promote the acceptance of Newton's work. Indeed the involvement in this project of other Newtonian scientists implies that they, too, felt the need for such a work, and were concerned that it be as convincing and as near perfect as possible.

In March, 1697, Gregory was in London and met Newton⁸. They discussed the brachistochrone and Gregory tried to understand Newton's proof that the curve was a cycloid. Other topics were discussed, but there is no mention among Gregory's notes of the meeting of astronomical topics. Yet it was in the following month that David began to plan his Astronomiae. It may well be that Newton (or perhaps another Newtonian met in London) suggested the topic to him. Perhaps, too, his failure with the brachistochrone inspired him to turn his attention away from mathematics towards astronomy. It may have been on this occasion that he finally realised there would be no second edition of the Principia, and so no publication of his Notae in the near future.

Whether or not the initial suggestion was Newton's he certainly helped Gregory to prepare the work. The two met several times, notably in February, 1698, June and July, 1698, March and early April, 1700, May, 1701 and July, 1701⁹. On almost every occasion, topics

⁸ London, 7.3.1697, A78.

⁹ February, 1698 A90 SUL, J.D. Forbes autograph collection no 45 and EUL, partly NCIV 584 265-6; June, 1698 A79 RG fo 62; July, 1698 A80 RG fo 62; 30.7.1698, A78; (July?) 1698, C62 NCIV 589 276-77; March, 1700 C121², RG fo 15; 1.4.1700, C122² RG fo 22; 21.5.1701 A68² with copy RG fo 74, partly NCIV 634 354-55; 3.6.1701 RG fo 79.

from the Astronomiae were discussed; Gregory invariably took Newton's advice and sometimes his words. Before publication, John Keill and John Arbuthnot read the manuscript and made many suggestions. The latter's were more stylistic than scientific, but Keill pointed out unstated assumptions and mutually dependent propositions. Gregory worked through his comments carefully, scoring them out as he either satisfied himself that the criticism was unjustified, or altered the manuscript in accordance with it. For example, when Keill raised doubts about a scholium Gregory intended adding to proposition 65, book 3 on tides, he decided to omit it altogether. Keill pointed out that the discussion in proposition 3, book 5 is valid only under the assumption of constant gravity, and Gregory included this caveat in the published version¹⁰. Halley discussed with Newton what should appear about the comet of 1680, and advised on the financial arrangements for publication¹¹. Flamsteed believed that Halley, too, had seen the work in manuscript¹². Indeed, the Astronomer Royal was notable in that he did not see the work before its publication.

The book then was not so much the work of one Newtonian scientist working on his own, but represented a concerted attempt by a group of Newtonians (including Newton himself) to elucidate the astronomical achievements of the Principia.

This interpretation of events clears up a puzzle in Newton and Gregory's relationship pointed out by Professor Cohen; how can we

¹⁰ Misc. 7.

¹¹ A68² partly NCIV 634 354-55.

¹² Flamsteed to Thornton: 18.2.1703. Francis Baily An account of the Rev'd John Flamsteed (London, 1835) 747-48.

explain Gregory's continued loyalty to Newton in the light of the unpublished Notae, to which Newton seems to have paid no attention at all?¹³ It seems that Gregory waited fourteen years, between his first suggestion of publishing the Notae in 1694 and his death in 1708, delaying publication of the commentary in hope of a second edition of the Principia. Over this period he showed no resentment at all that his Notae lay unpublished.

However, it seems now that Gregory exchanged one publication for another. Newton may have discouraged publication of the Notae, but he encouraged a far more glorious project. Instead of being the man who wrote a commentary on the Principia, Gregory became the man who wrote the first textbook of Newtonian science, in which the application of its principles to astronomy was clearly demonstrated. While the Notae would always have been dependent on, and subsidiary to, the Principia, the Astronomiae could stand on its own as a classic textbook of Newtonian astronomy.

It is easy to see why Gregory felt no resentment, and why, too, after that last note in March, 1696, he apparently no more considered publishing the Notae, even when it was clear that a second edition of the Principia would at last appear. On 25th March, 1708, Gregory noted that the new edition was now being printed at Cambridge¹⁴. Yet neither then, nor a year earlier, when he discussed the new edition and Newton's proposed changes in it, did he mention the Notae¹⁵.

¹³ Cohen op cit (3) 199.

¹⁴ E, inside end cover; Hiscock 41.

¹⁵ 15.4.1707 E78; Hiscock 40.

With the help of Halley, Keill and Arbuthnot, and, above all, the constant advice of Newton himself, Gregory had been enabled to set his name to the first official text of Newtonian science since the Principia itself. After this, publishing the Notae could only prove an anti-climax.

5.1.2 Sources for the Astronomiae

When Gregory drew up his first draft proposals for the Astronomiae, at 11.30 in the morning on 23rd April, 1697, he included a list of sources he would need to consult¹⁶. This list was later copied into workbook E with various additions¹⁷ and I have included the list as appendix to this chapter. These sources show at once how much wider the astronomical content of the work would be than that of Newton's Principia.

Around July, 1691, Newton wrote a paper for Richard Bentley, sketching out a preliminary reading list for study before the Principia should be tackled¹⁸. He included only two works of astronomy, Mercator's Institutionum Astronomicarum ... (London, 1676) and Gassendi's Institutio Astronomica ... (Paris, 1646). Even these did not need to be read entirely. He recommended only the account of the Copernican system at the end of the latter, and as much of the same system as is in the former, as well as its appendix on the new discoveries made by telescope. John Craige, writing to William Wotton in June, 1691, on the same topic mentioned no astronomy books

¹⁶ A60.

¹⁷ E 87-88.

¹⁸ c.a. July, 1691. NCIII 367 155-56.

at all¹⁹. Out of all those Gregory lists, Craige mentioned only James Gregorie's Optica Promota - and that for its optics and not its astronomical appendix. Of course, expounding a work for others requires a deeper background knowledge than simply understanding it for oneself, but the 28 authors Gregory lists take one far beyond the bounds of the Principia. Both the Notae and the Astronomiae might broadly be said to aim at interpreting and expounding Newton's Principia, but this reading list shows at once how much wider was the scope of the Astronomiae.

The emphasis Gregory places on Kepler's work is immediate. All his major astronomical works are cited, in which his three laws are set down clearly. Mercator's Institutionum also set out Kepler's laws and Horrox, whose work is included, was an enthusiastic proponent of Keplerian astronomy.

With the exception of Ptolemy's Almagest, all the works in Gregory's list are Copernican. In the Astronomiae, he refers, for example, to Riccioli's observations of twilight, his values for the obliquity of the ecliptic, his observations of the fixed stars and many other aspects of his work²⁰, but this anti-Copernican author has no

¹⁹ John Craige to William Wotton; 24.6.1691 NCIII 364 150-51.

²⁰ The editions of Gregory's Astronomiae are listed in appendix to Chapter 1. I have generally used Ast(26), with Ast(02) and Ast(1972) where necessary. However, the translation into English was not always quite accurate and care must be taken in using the English editions. I have given references so that, for example, Ast(26) 11 1 prop.3 208, is proposition 3, of section 1, page 208 of book 2, in Ast(26). See index to Ast(1972) here.

place in Gregory's list of basic source material. Stevin's work, which included an attempt to explain tidal motion by lunar attraction, was one of the earliest Copernican astronomies, and Gassendi's Institutio argued vigorously for Copernicus' theory.

The Keplerian works are not in such a clear majority, largely because of the difficulties involved in an application of Kepler's second law. The tables of Landsberg and Longomontanus were certainly not Keplerian, but were based on circular orbits. Mouton's tables were important less for their actual content than for their use of successive differences in establishing tables of numbers whose law of formulation is known. De la Hire's tables were empirically based and Gregory had previously criticized them for this²¹. In the Astronomia, de la Hire's value for the obliquity of the ecliptic is compared unfavourably with those of Hevelius, Riccioli and Streete, his method of calculating an eclipse is referred to but not described, Mouton's method of differences is referred to and Longomontanus' authority is cited for the term 'Copernican system'²². No other explicit use is made of these authors, nor of Landsberg.

More significance lies in the theoretical works chosen by Gregory, and here Bouilleau's work, the major rival hypothesis to Kepler's, is included. The tables based on his theory proved at least as accurate as Kepler's Rudolphine Tables - Sherburne considered them more so²³ - and they were certainly easier to use. Streete's tables

²¹ Notae 135.

²² Ast(26) 2 IV prop 19 285, ibid 4 VIII prop 48 628, ibid 5 III prop 25 756, ibid 1 XI prop 79 184.

²³ Edward Sherburne The sphere of M. Manilius ... (London, 1675) 96.

also followed this hypothesis (though with an empirical rather than theoretical justification for so doing) and Gregory had used these tables himself when he lectured on astronomy at Edinburgh. However, Streete stated Kepler's first and third laws and Ward regarded the area law, Kepler's second, as equivalent to the approximating equant device he employed. The others who used such devices, Mercator, Halley and Horrox, regarded them merely as aids to calculation.

Gregory used Hevelius' works mainly for his observational work, as he did Huygens and James Gregorie, although the latter two were also used for his sections on parallax. Sherbourne's translation of Manilius was included for the sake of its appendix, which, as Gregory remarks elsewhere

'mentions all that ever wrote of Astronomy'²⁴.

Fatio's and Cassini's theories of zodiacal light were given in the scholium to proposition 8, book 1. The work of Bernoulli, Hooke, Halley, Hevelius and others on the nature of comets was discussed in book 5. Dechales' work was the compendium of modern ideas on which Gregory had drawn extensively for his Edinburgh lectures. It contained a section refuting the hypothesis of Descartes and it is noticeable that Descartes' Principia is not on Gregory's list which otherwise contains most of the classic works of seventeenth century astronomy. The list did not include works such as this, or Leibniz's theory of the harmonic vortex, which Gregory would include in the Astronomiae only to refute.

²⁴ 1697, E90, Hiscock 5.

These works reflected the form of the book. It drew on a large number of authorities other than Newton, and was firmly Copernican. Kepler's work was praised and his laws accepted as indisputable, but space was also found for the approximating devices to his second law, such as Ward's and Bouilleau's.

5.1.3 Shaping the Astronomiae

Gregory's first draft plan of April, 1697, proposed a work of four books²⁵. These would deal in turn with the true (Copernican/Newtonian) system, the apparent (Ptolemaic) system, the general theory of the planets and comets, and the particular theory of each planet. Book one would be especially Newtonian in emphasis giving the Newtonian mechanics of revolving bodies and, by linking this to observed celestial phenomena, deducing the inverse square law of gravitation. The laws of vortices would be deduced and shown to be inconsistent with the observed notions in the heavens: in particular, Leibniz's paper on his harmonic vortex theory would be examined and rejected²⁶. Throughout the work, while due honour was paid to Newton, Kepler was to be given his place as the founder of physical astronomy. His Epitome would be used in this book, along with Gregory's Edinburgh lectures on astronomy to explain the appearances arising because of the earth's annual motion.

The second book would deal with the apparent system and the divisions of the heavens by, say, the ecliptic or equator, and their systems of secondaries and parallels. Part two of the Edinburgh

²⁵ A60.

²⁶ Leibniz 'Tentamen de motuum coelestium causis' AE (February, 1689) 82-96.

astronomy lectures, and the work of James Gregorie, David's uncle, would be used here. This book would discuss the projection of circles onto planes giving rise to conics and so explain astrolabes, gnomonics, analemmas and so on. Finally the analogous apparent astronomies as viewed from other planets would be described here.

Book three would consider the planetary orbits in a more practical way than the theoretical approach of book one. Here, the approximations used in calculation (that is, equant devices and their refinements) would be discussed, and the graphical determination of a comet's path shown. The moon would also be considered here.

The fourth book was to make these generalisations particular, and to discuss also such topics as the shape, zones, surface gravity, tides, satellites, and, as an afterthought, inhabitants of each planet. The nature and possible influence of comets was to be examined here, along with Whiston's theory of the flood²⁷.

Gregory's intention was to provide an introduction to Newton's astronomy, which would be useful to both learned and unlearned, and serve as a text book for those lecturing on the Newtonian system. The reader, wherever possible, would be referred to other works for geometrical theorems, but the astronomical work would be complete in itself. The work changed details of format and content over the following years, but in aims and in scope the Astronomiae which appeared in 1702 was the work Gregory envisaged in 1697.

This original scheme was tidied up and copied, with a slightly

²⁷ William Whiston New theory of the earth (London, 1696) suggested that all major changes in the earth's history (including the deluge) could be attributed to the action of comets.

enlarged booklist, into workbook E²⁸. Around this time Gregory began a running list of things done, or to be done, for the Astronomiae. One of his first notes here was

'To consult Mr Newton about the design, method and particular difficulties'²⁹.

He next met Newton in February, 1698, and on the 28th of that month he drew up a revised plan for the work³⁰.

Book one was to remain essentially the same, although universal gravitation and the production of planetary orbits from its combination with a lateral motion were now specifically mentioned. Kepler's physical theories, as well as Descartes' and Leibniz's were now to be examined and rejected. The doctrines of parallax and refraction were added to book two and the comparative astronomy removed. The third book would consider both general and particular theories of the planets, while book four discussed their satellites. This book would also consider tides and Saturn's ring with a description of the solar system seen from a satellite as Kepler had done in his Somnium. A fifth book was added to describe the motions of comets. As an afterthought Gregory noted that the inequalities caused in these motions by the effects of planets on each other are to be described

²⁸ E87-8.

²⁹ Misc. 70 consists of 27 pages of different sizes roughly stitched together, entitled 'Contenta mathematica in Actis Lipsiae advertenda in nostra Astronomia...'. As well as these contents of the Acta and the running list mentioned above it contains many other notes relevant to the Astronomiae.

³⁰ A43.

in the appropriate places.

This revised draft was drawn up in London, and it is tempting to conclude that the alterations were made at Newton's suggestion. Unfortunately, we cannot be sure of this, and it must remain simply a possibility. In particular, the possibility that Newton influenced Gregory's decision to refute Kepler's physical theories is discussed in section 5.2.1 below.

By August, 1699, Gregory had seen the advisability of separating out the remarks on planetary theory and expanding them into a sixth book. At this time he toyed with the idea of discussing the final causes of things along with Whiston's theories, but in fact said almost nothing on such topics³¹. These six books were essentially complete by February, 1700, at around which time Gregory visited London and received Newton's theory of the moon for inclusion in the work³². From then on Gregory concerned himself with polishing and revising the work, which he now lent in manuscript to Keill and Arbuthnot for their comments³³. These revisions continued throughout 1700, until by December he was considering omitting book six altogether, along with several of the more troublesome propositions from the other books. In June, 1701 he was to meet Newton and Halley in London and on 21st May he listed several last-minute points to raise with them³⁴. In particular he still wished to discuss Newton's

³¹ Misc. 70. For Whiston's theories see n27.

³² Cl21², RG fo 15.

³³ Misc. 7.

³⁴ A682 NC IV 634 354-56.

lunar theory, to discover what Newton and Halley wished him to omit about the comet of 1680, to ask for Newton's tables of refraction and to discuss Cassini's orbit. With Halley, Gregory wished to talk over the financial and practical arrangements of publication and to discover from him Flamsteed's 'present inclination' to himself and opinion of his Astronomiae. This last was determined only after publication. When Gregory met Newton on 3rd June, they discussed several astronomical topics, including the moon's orbit, and Gregory later received the refraction tables he had hoped for³⁵. The discussion between them then of the forces necessary to support the Copernican system and its rivals was used by Gregory at the end of book one of the Astronomiae.

However, Gregory's main problem at this final stage was the lack of a preface. As early as May, 1694, Newton had told him of his intention to exhibit the agreement of his philosophy with the ideas of the ancient astronomers³⁶. Then, or perhaps later, he had given Gregory notes of his own on this theme, probably first intended as commentary on propositions 4 - 9, book 3 of the Principia³⁷. This paper eventually formed the basis of the Astronomiae's preface, but unfortunately we cannot be sure whether this was at Newton's suggestion.

³⁵ RG fo 79; refraction tables A61².

³⁶ C44 RG fos 68, 9 NCIII 446 334-36.

³⁷ These papers and the insights they afford into Newton's thoughts are discussed in J.E. McGuire and P.M. Rattansi 'Newton and the "Pipes of Pan"' Notes and records of the Royal Society 21 (1966) 108-43, RG fos 1-9.

Cohen, for example, seems to imply that it was an unsanctioned publication which probably angered Newton³⁸. As he points out, it was becomingly modest for Newton to suggest that he had merely rediscovered truths known to the Ancients, but the same comments from Gregory constitute a belittlement of Newton's achievements. Certainly, Newton would have had every right to be angry if his papers had been so published without his consent.

Yet on balance it seems more likely that Newton had suggested the topic to Gregory, or at least agreed to Gregory's suggestion of it, as suitable for a preface. When he travelled to London in June, 1701, Gregory wished especially to consult Newton about a preface, and after he had met him, he began to plan it in this form³⁹. Unfortunately, although he added notes on the preface to his notes of his discussion of other topics with Newton, there is no way of confirming that the preface was also discussed at that time. He meant at first to consult Galileo Systema cosmographica (1635 translation of Dialogo (Florence, 1632)), Bouilleau Astronomia philolaica (Paris, 1645) Riccioli Almagestum novum (Bononiae, 1651), Dechales Musica (in Cursus ... (Lyons, 1674)) and Mersenne Harmony (Paris, 1635) as well as Newton's papers. In the event, Newton's remarks furnished virtually the entire preface. Certainly he had the desire to consult Newton on the preface and the opportunity to do so, but we cannot say definitely that the final form was agreed between them.

³⁸ Cohen, Ast.(72) xv-xvi.

³⁹ 21.5.1701, A68², NCIV 634 354-55; 3.6.1701 RG fo 79, n.d. paper after A66.

Early in 1692, Fatio had written to Huygens telling him that Newton believed he had detected that the Ancients had known all the major parts of his philosophy. As McGuire and Rattansi point out, this was probably written at Newton's instigation in order to test out Huygens' reaction, which was civil, but unenthusiastic⁴⁰. It would have been quite in character for Newton, wishing to gauge the general reception of these ideas, but unwilling to risk any personal criticism, to have encouraged Gregory to print them as his own in the Astronomiae. Any such scheme must have involved the implied criticism noted by Cohen, so long as Newton was unprepared to acknowledge the ideas publicly as his own. It is quite plausible to suggest that Newton recognised this as the price he must pay for testing reactions to these ideas while preserving his anonymity, and urged Gregory to use them for a preface.

Moreover, Gregory had been discussing all aspects of the work with Newton while he wrote it, asking his advice and his wishes on many points. In 1698 he had known Newton well enough to recognise that he would object strongly to Flamsteed mentioning in print the lunar observations he had supplied (see 1.9); he would have known full well in 1701 that Newton would have been justly angry if Gregory had published his paper without consultation and permission. He had previously been punished with 2½ years of silence for displeasing Newton (see 4.1); why should he run the risk of a recurrence by not determining Newton's feelings on this point, when he had already discussed with him many other matters affecting him less closely?

⁴⁰ Fatio to Huygens: 5.2.1692 NCIII 193; McGuire and Rattansi op cit (37) 109-10.

Even if Newton did not suggest the topic, Gregory surely had the sense to ask his approval before publication.

Most significantly, Newton was not angry. Only the remarks of Thomas Hearne suggest that he was, and these are discussed and refuted in 5.8. As Hiscock has pointed out, Gregory's memoranda evidence his continued friendly relationship with Newton and show at once that Newton was not displeased with the Astronomiae⁴¹.

All in all, it is quite possible that Newton suggested this topic to Gregory when he came to consult him in June, 1701 over the form of a preface. If not, it seems almost certain that Gregory at least asked Newton's approval before selecting this topic himself. It is highly improbable that Newton learnt of this preface only when he saw the printed textbook; Gregory had a better knowledge of the possible consequences of such an occurrence to allow it to take place, and Newton would not have taken it so complacently if it had.

Thus, as well as the numerous places where Newton had advised him on form and content, Gregory's Astronomiae contained two pieces, the preface and Newton's lunar theory, which had been originally intended for a second edition of the Principia. Its importance, not only to Gregory, but to Newton himself and to Newtonian science as a whole, could not be more clearly underlined. The manner in which Newton and Newtonian disciples helped to shape the work is clearly revealed in a selection of themes discussed in the following sections.

⁴¹ Hiscock, viii.

5.2 Book one: the Newtonian system

This book is based on sections 1-3 and 11 of book one of Newton's Principia; that is, on Newton's analysis of bodies moving in conic sections urged by a central force reciprocally proportional to the square of their distance from a focus of the conic. Gregory, however, begins with an account of the Copernican system and thereafter relates each result empirically to the heavens. He ignores such aspects of Newton's work as the extension to forces bearing other ratios to distance, although he had at one time considered some such a discussion. A paper among his manuscripts examines a body moving in an ellipse under a centripetal force tending towards its centre, and varying directly as the distance from that centre. It was originally intended for the Astronomiae, but he has noted that the propositions it contains may be omitted as they are of no further use in the work⁴². Other similarly theoretical studies of no immediate application may have been deliberately omitted, but we have now no trace of them.

As well as explaining the Newtonian analysis of the Copernican universe, he examines and rejects alternative viewpoints. The final section examines the various alternatives to the Copernican and was probably added at the last minute after Gregory discussed these points with Newton in June, 1701. In the Ptolemaic system the sun and all the planets revolve about a stationary earth; in the Tychonic (geometrically equivalent to the Copernican) the sun revolves about a stationary earth while the other planets revolve about the sun; the

⁴² Paper after C206.

semi-Tychonic is similar except that only the inner planets, Venus and Mercury, revolve about the sun, while the outer ones revolve about the earth. Newton and Gregory had discussed the forces necessary to maintain each of these systems, and the Astronomiae enlarged on this discussion, concluding that the Copernican, requiring the simplest configuration of forces, should be adopted. More extensive was the discussion in the preceding section of alternatives to the Newtonian analysis.

5.2.1 Alternatives to Newtonian cosmology

In section X, book 1, Gregory examines the physical theories of the universe proposed by Kepler, Descartes and Leibniz, and rejects them for the Newtonian viewpoint.

Gregory's original draft plan for the work, of April, 1697, had intended an examination in this way of the systems of Descartes and Leibniz. The impossibility of the Cartesian vortex theory had been a theme of the graduation theses delivered by his brother, James Gregorie, in 1690 (see 2.3.3) and he may already have written his paper, unfortunately now lost, which criticized the views proposed by Leibniz's 'Tentamen' in the 1689 Acta⁴³. At that time, however, although Descartes was to be shown to have done nothing worthwhile in physical astronomy, Kepler was to be presented wherever possible as its founder.

Yet, in February, 1698 when he drew up a revised plan after meeting Newton in London, Gregory made no such glowing references to

⁴³ Leibniz op cit(26); Gregory's paper was A6.

Kepler⁴⁴. Instead his physical system was to be examined and rejected alongside those of Descartes and Leibniz. Gregory's admiration for Kepler's work had not blinded him to the imperfections of his physical theory; in December, 1693, he had noted that the direction of cometary paths, contrary to those of the planets, was an argument against Kepler's system as much as against Descartes⁴⁵.

His criticism of Kepler appears reluctant. After describing his quasi-magnetic theory of planetary motion in full⁴⁶ Gregory remarked,

'Tho' we undertake to oppose Kepler's Celestial Physics, yet we don't do it with such a temper, as if we rank'd him (whose Fame will be Immortal) among the common System - makers or hunters after Physical Causes. For he was so far from this, that on the contrary, he is the only Man (excepting perhaps some of the Ancients, as Pythagoras etc.) who has treated of the Celestial Physics in a Mathematical manner⁴⁷

Gregory introduced Horrox in praise of Kepler and added his own further praise of the man who discovered the elliptical paths of the planets and the area law. Only then did he turn to criticism of his physical theory.

⁴⁴ A43.

⁴⁵ Notae 185.

⁴⁶ These theories are found in Kepler's Epitome Astronomiae Copernicanae ... 3pt (Lentiis ad Danubium Francofurti, 1618-22) bk 4.

⁴⁷ Ast(26) 1 X prop. 70 145.

Gregory was himself unwilling to discuss Kepler's theory and decided at first to pass it by in silence. The continental vortex theories were, in any case, more dangerous (because more popular) rivals to Newtonianism than Kepler's theory. It would have been quite reasonable to discuss these alone. However, something caused Gregory to change his mind and reject Kepler's theories too. Unfortunately, the only notes we have of Gregory's meeting with Newton in February, 1698, do not mention the Astronomiae. It is plausible to suggest that Gregory's plans were discussed on another occasion and the revised draft of that month then drawn up. It may have been at Newton's suggestion that Kepler's theory, too, was argued against; not only the major, but all, alternatives to the Newtonian system must be rejected. However, unless further notes of their discussions turn up, this must remain a conjecture. It would explain, though, Gregory's discussion of Kepler's theories in the face of his reluctance to criticize 'the only Man who has treated of the Celestial Physics in a Mathematical manner'.

Cohen also emphasises the importance of Gregory's treatment of Kepler in introducing his physical astronomy to a wider audience. With the translation of the Astronomiae in 1715, this work became the first in English to give any such thorough introduction of Kepler's work⁴⁸. Of course, this applies not only to this discussion of Kepler's theory of the motion of the planets. The Astronomiae, as Gregory intended, cites Kepler's opinion on many matters; for example, it gives his theories on comets and their tails, states his laws and

⁴⁸ Cohen, Ast(72) xvii-xxi.

gives an extended account of his description of the universe as viewed from the moon. This discussion in book two may have been introduced by Gregory for the sake of completeness in presenting Kepler's work.

Newton's other popularizers, as Cohen points out did not generally discuss Kepler's work in any detail. In this, they followed Newton himself, who rarely mentioned his predecessor. Gregory had criticized Newton on this point in his Notae, and Cohen comments

'a token of Gregory's independence of spirit in this work is that he should have been willing to take so firm a stand for Kepler against Newton'⁴⁹.

However, 'against Newton' in this context only means in distinction to Newton's treatment of Kepler in the Principia. Gregory was certainly not taking a stand against Newton by preferring Kepler's work to his; Newton's theory of comets and Newton's cosmology were preferred over Kepler's. As discussed in 5.4.1, Gregory does not seem to have fully accepted Kepler's laws until they were made part of the Newtonian synthesis.

It is plausible that Gregory, realising his Astronomiae would present a much more favourable picture of Kepler than had the Principia, introduced a critique of his physical laws as a sop to Newton. Perhaps, having been persuaded by Newton to criticize this aspect of Kepler's astronomy, he felt free to praise its other aspects. Or perhaps it was never discussed between them, and this critique was included only for the sake of

⁴⁹ Ibid xxi.

completeness. Whatever the reason for its inclusion, this discussion, along with other less extensive references to Kepler's work, meant that the Astronomiae not only introduced the Newtonian cosmology, but also presented Kepler's astronomy to a new British audience.

In the event, Gregory criticised Kepler on four main points. Kepler's planets maintain a circulatory motion by virtue of a propelling power emanating from the sun. They move to and from the sun in accordance with the attraction and repulsion of their poles which alternately face the sun. Gregory criticises Kepler for allowing a body showing neither pole to the sun to describe a uniform circle; a body propelled only by the sun's 'vectory power' should move in a straight line. By a similar argument, a planet moving from perihelion to aphelion with its repulsive pole to the sun should follow a curve convex to the sun. Thirdly, Kepler assumes that the sizes of the planets are as their distances from the sun, and their densities reciprocally as the square roots of their sizes. Hence their masses are as the square roots of their distances from the sun, which is necessary for Kepler's deduction of his third law. But these assumptions as to planetary size are manifestly contradicted by observations. Finally, Kepler's magnetic virtue which has

'too little of Mechanism, and too great an affinity with
an Animal Power'

and his fibres of libration and longitude, as well as his division of each planet into a kernel and completely separate shell are all

'foreign to the Method which Nature uses'⁵⁰.

Besides these, though, Gregory has another criticism. Kepler's attractions and repulsions, not being mutual to sun and planet contradict the law of equal and contrary action and reaction. Yet this is in spite of Kepler's own earlier remarks in the introduction to his Astronomia Nova (Prague, 1609) which described gravity as a mutual effect proportional to mass, and identified the moon's gravity as the cause of tides. (Of course, these criticisms are unfair, as Kepler saw the cause of the planets' motions as something separate from gravity, and did not consider equal and opposite actions and reactions as a universal law.)

Gregory concludes his discussion of Kepler's theory by quoting in full these comments on gravity as a mutual effect and the moon as the cause of tides. His conclusion does not emphasise how far wrong Kepler was in his physical theories, but how close he was in these instances to Newtonian science.

Gregory gave Descartes' theory a very different treatment. His vortex system is also fully described from part 3 of his Principia, but it is presented from the first as a retrograde step. Kepler had understood that a simple vortex was not enough to explain the planetary motions, but Descartes

'making light and taking no notice of these niceties and Astronomical Observations, and being resolved to frame a World, brought in Vortices again'⁵¹.

⁵⁰ Ast(26) 1 X prop. 70 148.

⁵¹ Ibid 151.

After describing the system, Gregory introduces Newton's proposition 52, section IX, book 2 of the Principia on periodic times in a vortex formed by the rotation of a sphere. This result applied to the Cartesian system contradicts the observations. A vortex also necessitates a constant influx of fresh motion at the vortex centre, means that all planetary orbits will accumulate in a plane perpendicular to the axis of rotation and cannot allow the passage of comets in a direction contrary to that of the planets. Using these and similar arguments, Gregory concludes with Newton that this system confuses rather than explains the celestial motions⁵².

Gregory had, however, a much higher regard for the vortex system of Leibniz⁵³. The velocity of the 'harmonic vortex' of this system was divided into two components; a radial velocity and a velocity of circulation perpendicular to it. This vortex rotates so that its velocity of circulation is inversely proportional to the distance from its centre of circulation, which, as Kepler had shown, meant that any planet carried by it obeyed his distance law. The planets also had a radial velocity produced by gravity towards the sun, combined with the centrifugal force of their motion. The gravity was produced by a second vortex which circulated in accordance with Kepler's third law. Leibniz deduced that to produce elliptical orbits the gravitational attraction must obey the inverse square law.

As Dr Aiton has discussed, the law of Leibniz's harmonic vortex

⁵² Ast(26) 1 X prop. 76 172; see I. Newton Philosophiae naturalis principia mathematica (London, 1687) book 2, 400.

⁵³ Leibniz op cit(26) is discussed by Eric J. Aiton The vortex theory of the planetary motions (Belfast, 1972) 125-51.
London

does give the correct area law and must be distinguished from the incorrect distance law, which states merely that a planet's velocity is inversely proportional to its distance from the sun⁵⁴. Kepler's Astronomia Nova had given first the incorrect form of the law, but later its correct form. His subsequent statement of this correct form in the Epitome was less ambiguous. Gregory's acceptance of Leibniz's claims for his vortex is further evidence of his familiarity with Kepler.

When first planning his Astronomiae, Gregory remarked that Leibniz's system was worthy of inclusion as it holds true for any one planet; unfortunately, as Gregory had shown in his, now missing, paper on the system, it falls down when applied to several planets together⁵⁵. This remark, with an oblique reference to Herigone's statement of Kepler's third law⁵⁶, suggests that Gregory must have pointed out in this paper at least one of the main problems in Leibniz's hypothesis; the vortex giving the planets gravity circulates in accordance with the third law but the harmonic vortex, which actually carries them round, does not. This brief description of the contents of the missing paper also mentions specifically paragraph 17, p.90 of Leibniz's 'Tentamen' and this is quoted in the Astronomiae as evidence that Leibniz intended his theory to apply to more than one

⁵⁴ Eric J. Aiton 'Kepler's second law of planetary motion' Isis 60 (1969) 75-90.

⁵⁵ Misc. 70; the missing paper was A6.

⁵⁶ Pierre Herigone Cursus mathematicus 6 books (Paris, 1634, 1637, 1642) 5 574. J.L. Russell 'Kepler's laws of planetary motion, 1609-1666' British journal for the history of science 2 (1964-65) 1-24 believes that this is the first published citation of the law outside Kepler's own works.

planet of a vortex⁵⁷. It seems probable that Gregory's original comments on Leibniz's paper were not very different from those he later published in the Astronomiae.

As he had done for Kepler and Descartes, Gregory fairly described the system of the Tentamen, giving none of the mathematics, but detailing the physical assumptions made. Unlike Newton and Keill some twelve years later⁵⁸, Gregory apparently had no doubts over either Leibniz's concept of centrifugal force, or his use of second order infinitesimals. Gregory accepted all the mathematical analysis of the 'Tentamen', saying that, granted the harmonic vortex,

'all the rest, which our author draws from thence by Geometry, proceeds very well and justly, as things usually do under his Hand'⁵⁹.

In Gregory's view, the system labours first under the disadvantage common to all vortices; the inability to satisfactorily explain the motion of comets in a direction contrary to that of the planets. Its chief problems, though, as Leibniz himself seemed to have acknowledged⁶⁰, were that it did not explain the mechanism of gravity and, even more immediately, that the problem of satisfying Kepler's third law could only be solved by setting each planet in its separate

⁵⁷ Ast(26) 1 X prop. 78 180.

⁵⁸ For the reactions of Newton and Keill to Leibniz's 'Tentamen' see E.J. Aiton 'The celestial mechanics of Leibniz in the light of Newtonian criticism' Annals of Science 18 (1962) 31-41.

⁵⁹ Ast(26) 1 X prop. 78 178.

⁶⁰ Leibniz op cit(26) 96.

vortex, an unattractive system with difficulties of its own. Gregory explained why both the Leibnizian and Newtonian analyses give rise to a centrifugal force obeying the inverse square law; in Leibniz's system the combination of harmonic vortex and the centrifugal force which balances the gravitational attraction produces just such a tangential motion as Newton assumes in combination with his centripetal force.

This seems to have been the first public comment by a Newtonian on Leibniz's harmonic vortex, and Leibniz answered it in 1706⁶¹. He was, however, unable to answer Gregory's arguments very convincingly. Without naming him, he claimed that Gregory was unable to perceive the force and usefulness of the concept of an harmonic vortex, which produced the effect of a body moving as if in a vacuum. Agreement with Kepler's third law is achieved by restricting harmonic circulation to the planetary orbs within which each planet revolves, and which are of negligible thickness compared to the entire vortex. He says nothing of comets, and argues that the inverse square law arises, because, as is well established, it is the natural law for virtues of any sort propagated from a source. His derivation of it is further proof of the correctness of his system, and not, by implication, a consequence of its mathematical identity with Newton's, as Gregory suggested.

Gregory did not see this answer until within three weeks of his death, when, on 20th September, 1708 he recorded that he had seen it;

⁶¹ AE (October, 1706) 446-51.

'it is a very poor paper, and does not so much as touch the main difficultys, but acknowledges all it touches'⁶².

He mentioned no plans for a reply, but, in any case, he died before he could have produced one.

Gregory argued convincingly that Leibniz's system was untenable, but, unlike his criticisms of Descartes for ignoring the niceties of astronomical observations, his discussion of Leibniz's hypothesis was generous. He freely acknowledged that if any could make a vortex system tenable, Leibniz could⁶³. (Of course, in saying this he made his dismissal of Leibniz's system a dismissal of all vortex systems, but he does not pursue such an argument.)

He was careful to explain that his discussion of these three rival systems to the Newtonian took no account of final causes, but was concerned with only those effects we can see and measure. No attempt was made to provide such an explanation of gravity as Fatio had been at pains to develop; the basic concept of terrestrial gravity extended to the heavens was sufficient for Newton's system which stood without any further explanatory mechanism. The two main attempts, of Kepler and Descartes (and the latter as extended by Leibniz), to provide a mechanism for planetary motions have been shown by Gregory to be unacceptable. He had criticised Kepler for employing a magnetic virtue which was less a mechanistic explanation than an animal power. Yet he showed no hesitation in accepting the Newtonian system based on the similar and unexplained virtue of universal gravitation.

⁶² 20.9.1708, E89 Hiscock 42.

⁶³ Ast(26) 1 X prop.77 173.

5.3 Book two: the apparent system

The second book deals with the apparent, or Ptolemaic, system of the heavens. Gregory explains that, as he had done in the astronomy lectures he gave at Edinburgh, he has inverted the usual order by presenting the 'true' system before this apparent one. This is so that the student becomes accustomed from the first to considering the earth in motion.

This book covered pretty much the same ground as had parts two and three of the astronomy lectures at Edinburgh. Gregory explained the division of the sphere of the fixed stars by various circles and their secondaries and parallels, and the use of celestial and terrestrial globes to solve related problems, with many worked examples. He discussed the division of time, both civil and astronomical, adopted by various societies, determinations of the positions of the fixed stars and problems of the first motion with a description of tables showing this in the stars. In section one, the strange lights seen by Fatio and Cassini were discussed and in section six he used spherical trigonometry to evaluate the sun's parallel at minimum twilight. Sections seven and eight deal with parallax and the refraction of light by our atmosphere.

Section four examined methods of measuring the obliquity of the ecliptic. Gregory gave the measurements of the ancient astronomers which seem to indicate that the obliquity has been slowly decreasing since their time. However, most contemporary astronomers (Gregory listed Gassendi, Riccioli, Horrox and Hevelius) believed the value to be constant and explained the ancient measurements as the result of faulty instruments. Gregory remained uncommitted on this point, and accepted a value for the obliquity at

that time of $23^{\circ}30'$. Section five followed with a discussion of observation of the fixed stars. Gregory praised especially the work of Brahe and Hevelius, and looked forward to the appearance of Flamsteed's catalogue. He listed values that astronomers had given for the precession of the equinox, and settled on an annual motion of $50''$

Gregory's treatment of these points serves to underline the difference between his approach and Flamsteed's. In his Gresham lectures the Astronomer Royal had also examined the various values which had been proposed for the obliquity of the ecliptic and the precession of the equinox⁶⁴. However, his treatment was far more extended; he considered far more measurements than Gregory did, even making use of data contained in Persian manuscripts supplied to him by Edward Bernard⁶⁵. Moreover, he studied the instruments and methods used in establishing them. Thus he was able to comment on the degree of accuracy obtained and so to arrive at a value of $23^{\circ}29'$ for the obliquity and $51''$ for the annual precession. He argued for a constant figure in each of these cases, showing that this was consistent with the available observations. Gregory had ventured no opinion in the first case, and had dismissed the possibility of trepidation in the precession without discussion.

Flamsteed's was the approach of the practical astronomer. First, the determination of such constants was a matter in which he was himself involved, and which he regarded as highly important. To Gregory,

⁶⁴ Bernard to Flamsteed: 14.8.1681 PT XIV (September, 1684) no 163, 721-25.

⁶⁵ The Gresham lectures of John Flamsteed, edited and introduced by Eric G. Forbes (London, 1975) Lectures 10-14, 28-32, 34 see introduction pp.46-50, 62-65.

who left such determinations to others, accuracy in these values was of course highly desirable, but was not a matter worthy of lengthy discussion. Moreover, Flamsteed's practical experience had enabled him to examine the instruments and methods of others and so to comment on their accuracy. Such an examination would have been beyond Gregory's reach. It was in this contrast between theoretical and practical astronomer that much of their antagonism towards each other lay. Their personal disagreements might be related in a large measure to their different relationships with Newton, but there was also a strong element of professional disagreement which arose from their different approaches to astronomy.

5.3.1 Atmospheric Refraction

The problem of atmospheric refraction was a relatively recent one for astronomy. When a beam of light enters the atmosphere, it is continually deflected by refraction, with the result that objects in the sky appear to be higher than they in fact are⁶⁶. In the late sixteenth century Tycho Brahe had produced the first empirical tables of refraction, and in 1610 Kepler built up the first tables based on an a priori analysis. Unfortunately, though, he did not know the correct law of refraction. Snell's law had been made public by Descartes in 1637, and on this basis, in 1662 Cassini built up a table from theoretical considerations. He assumed an atmosphere of uniform density (so that the ray of light was refracted only once on entry) and limited height, which worked fairly well up to a zenith distance of 80° .

⁶⁶ This discussion of early tables of atmospheric refraction and of the efforts of Newton and Flamsteed is much amplified in ibid 65-9 and 'Note on atmospheric refraction' NC IV 96-97.

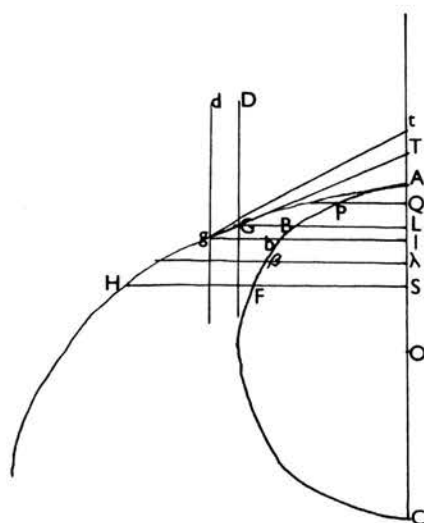
Flamsteed and Newton recognised the importance of this problem, and the latter sent refraction tables to the Astronomer Royal in 1694. There are two major assumptions that must be made before tables can be built on a theoretical basis. First we must establish the relationship between the air's density and its refractive power (refractive index - 1), and then that between density and altitude. The first two quantities were generally assumed to be proportional, but the second question was more difficult. These first tables of Newton's were based on a division of the atmosphere into innumerable concentric rings, whose uniform density was increased by uniform increments as distance from the earth similarly decreased. Flamsteed agreed that this was a reasonable theoretical basis.

However, by January, 1695, Newton had realised that his model implied that the air's refractive power was the same at the bottom as at the top of the atmosphere. Also, as Flamsteed pointed out, his tables did not altogether accord with observation. Using more of Flamsteed's observations, Newton, in early 1695, drew up another set of tables. This time he used proposition 22, book 2 of the Principia, wherein he showed as a consequence of his law of universal gravitation that if distances from the centre of the earth are taken in harmonic progression, the densities at those distances will be in geometric progression.

In February, 1695, Gregory learnt, probably from Edmond Halley, that Newton's first refraction tables had not agreed with Flamsteed's observations, and that he was now preparing a new basis for his tables⁶⁷. However, he does not appear to have learnt anything more of

this new basis, for in December, 1697, he attempted his own analysis on a different basis⁶⁸.

In the figure (a simplified version of Gregory's) let C be the centre of the earth, and SH its surface, topped by the atmosphere $SLGH$ divided into horizontal strips of infinitesimal width and



uniform density. For a ray TG , entering the strip $LGgl$, let $\sin(\text{angle of incidence}) : \sin(\text{angle of refraction}) = CL : Cl$.

Gregory claims that on this model the ray TG will be curved into the cycloid GgH , to which TG is the tangent at G .

Gregory's argument is essentially as follows:

Let ABC be the generating circle of the cycloid AGH .

Then $\sin \hat{TGD} : \sin \hat{tgd} = CL : Cl$ (by hypothesis)

$= CB : Cb$ (by assumption since lL is minimal) *

$= \sin \hat{BAC} : \sin \hat{bAC}$ (by geometry of semicircle ABC).

But, by the nature of a cycloid, TG is parallel to AB ,

$\Rightarrow \hat{TGD} = \hat{BAC} \Rightarrow \hat{tgd} = \hat{bAC}$.

⁶⁷ 3.2.1695 A40, RG fo 96 partly NCIV 492 82. The paper mainly contains notes of Gregory's discussions with Halley in January, 1695, with this note on Newton's refraction tables at the end.

⁶⁸ 28.12.1697 A100.

Hence the next infinitesimal arc of the refracted ray is parallel to bA.

But so is the next infinitesimal arc of the cycloid GgH, and thus the two infinitesimal increments coincide. By an extension of this analysis the curve of the refracted ray and the cycloid are the same curve.

However, apart from the assumed rectilinearity of the earth's surface, which means that the result could, in any case, be only approximately true, the assumption (*) is invalid.

In the semicircle ABC, if we call the radius r , and the centre O,

$$\begin{aligned} BC^2: bC^2 &= BL^2 + (r+OL)^2: bl^2 + (r+Ol)^2 \\ &= BL^2 + OL^2 + r^2 + 2r.OL: bl^2 + Ol^2 + r^2 + 2r.Ol \\ &= 2r^2 + 2r.OL: 2r^2 + 2r.Ol = r + OL: r + Ol = CL: Cl. \end{aligned}$$

Hence BC: bC = $CL^{\frac{1}{2}}$: $Cl^{\frac{1}{2}}$ \neq CL: Cl, and the infinitesimal width of the strip GgLL can in no way validate Gregory's claim. Indeed, we have here yet another example of Gregory's tendency to make unjustifiable claims of equality between infinitesimal quantities or between other related to them.

In this context, though, it is Gregory's model of atmospheric density which is of interest. He must have assumed the proportionality of density to refractive power, for this was so widely done that any alternative assumption would have necessitated stringent justification. Thus his assumptions about refractive index in his infinitesimal strips are assumptions about atmospheric density.

Suppose $a_0, a_1, a_2 \dots$ are the distances CS, C λ , Cl ... above the earth's centre, and μ_n is the refractive index of the strip bounded by lines of altitude a_n and a_{n-1} . Then we can write Gregory's

assumption as $\mu_n = a_n/a_{n-1}$. That is, the absolute value of the refractive index of the air at any altitude is dependent on the ratio of two terms in an arbitrarily chosen sequence. (In Newton's models, or course, it is the relationship between two refractive indices which depends on the relationship between two such terms. Neither of these relationships are absolute quantities at a given altitude, as refractive power is in Gregory's model, but both depend on the sequence of a_i chosen.)

The drawbacks of this assumption are clear; however we choose our series, as we decrease the distance between our a_i , μ_n will approach 1 for any n . In that case, refractive power will be zero, and the earth will be in a vacuum! We can approach this limit in different ways; if the a_i are in arithmetical progression the corresponding densities decrease as altitudes increase, but if the a_i are in geometric progression we have a constant refractive index at all altitudes. Thus these assumptions could only be valid if taken to refer to strips of finite width, bounded by very carefully chosen limits, which procedure could only be justified by correlation with observation and so would give rise to an empirically based table. But the whole of Gregory's cycloid analysis depends on the fact that these strips are of an infinitesimal width, which leads directly to the earth being in a vacuum. Gregory had clearly not examined the physical consequences of his analysis in any great detail. (Apart from considerations of agreement with reality, this examination would have shown him that a light ray should pass through such an atmosphere in a straight line, not a cycloid. This would have pointed out the existence of an error in his mathematics.)

There is no evidence on the paper of how Gregory arrived at the formula $\mu_n = a_n/a_{n-1}$. It is a neat copy, and may represent a final version after many other models had been tested but rejected for their failure to lead to a neat geometrical solution.

It may have been of special significance, however, that the curve was a cycloid. Earlier in 1697, Gregory had had this curve forcibly brought to his attention as the curve of quickest descent (see 4.5). He may have seen some analogy between Fermat's analysis of simple refraction as the swiftest path for a light ray, and his own of atmospheric refraction as the curve of quickest descent. This point is amplified in the conclusion.

If Gregory was predisposed to see the cycloid in this rôle, he may even have worked backwards towards a satisfactory model. However in this case he would surely have used $(CL/61)^{\frac{1}{2}}$ as his measure of refractive index. Most probably he tried several different models, which seemed likely to promise mathematically productive consequences, but perhaps with an especial lookout for a model which led (or which he could persuade himself led!) to the cycloid.

If we ignore the error in Gregory's derivation, this result could be seen as an interesting development of the geometry of the cycloid, based on a physically invalid, but mathematically pleasing model of refraction. Yet Gregory did not see his analysis as a mathematical game. He examined briefly the effect which this cycloidal refraction would have on the appearance of luminous phenomena, and suggested the construction of tables based on this hypothesis. To him, it was a serious attempt on the astronomical problem of atmospheric refraction, and may (though there is no sign other than the coincidence of dates

that this was so) have been intended for the Astronomiae.

In his Edinburgh and Oxford lectures, Gregory had mentioned the problem of atmospheric refraction, but had given only qualitative accounts of it. The basic phenomenon of refraction was left in the former case to the accompanying optics lectures, and in the latter to the works of writers on optics. Neither set of lectures mentioned refraction tables. In February, 1698, only two months after his own attempt on producing a curve for atmospheric refraction, Gregory met Newton in London and refraction was their major point of discussion - as far, at least, as can be judged from Gregory's surviving notes⁶⁹. The revised draft of the Astronomiae which Gregory drew up in that month was the first to mention refraction⁷⁰.

On this ^coccasion Gregory saw Newton's refraction tables, and wrote down a description of them. He noted

'[Newton] cannot do the refractions by precise Geometry'⁷¹.

The description Gregory gives of the theoretical basis for Newton's tables is very vague. He mentions only that the atmosphere is divided into layers of uniform density and that below 3° refraction is changeable and influenced by temperature for which allowance must be made.

But Newton had known since February, 1695 that even his second approach to the problem would not yield totally accurate refraction

⁶⁹ A90 SUL.

⁷⁰ A43.

⁷¹ A60 SUL.

tables⁷². There seems to be no obvious reason why he should wish to discuss the problem with Gregory three years later. Most probably, thinking of his own cycloidal refraction curve, Gregory had asked Newton about his approach. Perhaps he then told Newton something of his own analysis and was dissuaded from continuing, or perhaps the knowledge that even Newton could not perfect an a priori analysis was enough to persuade him of the futility of his own attempt. In any case, Gregory never returned to his attack on refraction.

Yet he wished to include refraction tables in his Astronomiae. On 1st June, 1698, he resolved to print those from the *Connoissance des Temps*, corrected from what he remembered of Newton's tables⁷³. It was probably around this time that he decided to ask Newton the precise refractive index he assigned to each layer of the atmosphere⁷⁴. When he visited Newton in June, 1701, he was still concerned with this problem, and finally Newton gave him a copy of his tables⁷⁵.

Yet even with these tables to draw upon, Gregory did not publish refraction tables in the Astronomiae. Nor did he give any a priori basis on which such tables might be built. As Flamsteed and others had pointed out, he said that the hypothesis introduced by Cassini, that a light ray is bent once on entering the atmosphere and then continues straight, was most unlikely. He maintained, as was generally believed, that light rays are almost certainly bent in a continuous curve as they travel through the atmosphere, whose refractive power

⁷² Newton to Flamsteed: 16.2.1695: NCIV 494 86-88.

⁷³ A56³.

⁷⁴ Misc. 70.

⁷⁵ A61².

varies with altitude. If we knew the nature of this relationship, we could build up refraction tables geometrically. We do not, though, know its precise nature, and we are hampered by factors related to time, season and so on, which affect refraction. Therefore, he concluded, we must build up our tables by combining theory with observation. He suggested that we divide the atmosphere into 8 or 10 layers of uniform density, each rarer than the one below, and find by comparison with observation the refractive index to be assigned to each. But (although he assumed a known relationship between density and refractive index, so that to have one was to have the other) he made no mention of the models used by Newton or any-one else to assign a relationship between altitude and density.

On reading Newton's Optics⁷⁶, Gregory resolved on 19th February, 1704 to alter ~~the~~ ^{the} scholium ^{to proposition 66, book 2 of the Astronomiae} to bring it into line with Newton's proposition 10, book 2⁷⁷. This proposition deals with the relationship between refractive power and density of a medium, and comments on atmospheric refraction. Newton says that when a light ray is refracted through several media enclosed by parallel surfaces, the final direction of the refracted ray is the same as if it had passed directly from the first medium to the final one. Thus atmospheric refraction may be treated as if light passed directly from a vacuum into the lowest, and densest 'layer' of atmosphere⁷⁸. Gregory realised that this did not invalidate his original discussion, but decided to add it at the end. In August, 1704, he still intended to make this addi-

⁷⁶ Isaac Newton Optics: or a treatise of the reflexions, refractions, inflexions and colours of light ... (London, 1704).

⁷⁷ Oxford, 19.2.1703/4 E113.

⁷⁸ Op cit(76) book 2 proposition 10 73-74.

tion, but remarked that it was not essential, as were the changes to be made in the proposition about Cassini's orbit⁷⁹.

Nowhere, however, does Gregory mention any intended alterations which would mean the insertion of tables of refraction. Why were these tables not included, and why did he suggest no geometric model on which they might be based?

As Gregory had known since February, 1698, Newton was dissatisfied with even his second attempt at a geometrical model⁸⁰. Perhaps simply through knowing this, or perhaps after discussing his model with Newton, Gregory was not prepared to publish his own analysis. Thus he was not in possession of a suitable model when the Astronomiae was published. The book did, however, suggest a semi-empirical method of construction for refraction tables and the Newtonian tables could well have appeared beside this. It may be that Gregory was reluctant to publish tables without a rigorous geometrical basis, in a book which purported to show how Newtonian analysis could handle all celestial phenomena. It seems more likely, though, that Newton was still unsatisfied with his tables, and while prepared to allow Gregory a copy for his private use, was not prepared to have them made public. Gregory, of course, would have been most unlikely to publish the tables constructed by the French when Newton's were not completed. On 10th October, 1698, Flamsteed had written to Colson that he had not Newton's permission to pass on his table of refractions, adding 'I

⁷⁹ Oxford, 2.8.1704 E127.

⁸⁰ A90.

believe he will see cause to withdraw it'⁸¹. Newton never withdrew it altogether, but it was not until 1721 that he allowed Halley to publish it in the Philosophical Transations⁸².

We cannot now be sure why the tables were not published by Gregory. However, it seems most likely that Newton still hoped in 1702 to perfect the tables and did not wish the incomplete version he had then to be made public. However, by 1721, he had resigned himself to the fact that they were as accurate as he could make them, and agreed to their publication.

⁸¹ Flamsteed to Colson: 16.10.1698 NCIV 594 284-85.

⁸² PT 31 (May - August, 1721) no 368 169-72.

5.4 Book Three: theories of the planets

This book considers theories of the planetary motions. The first section considers Kepler's second law, and the problem of determining areas which arise out of it (Kepler's problem is discussed in 3.3.3). It looks at approximating devices and the apparent compromise embodied in Cassini's curve. These are discussed below. The following sections determine the particular theories of earth and the other planets, explain tables of their motion and look at their size and density. In section six, methods of measuring the distance of a planet are examined. Book one had deduced a priori that the earth and planets are spheroidal. Now, section eight of book three shows how to measure the ratio of the earth's polar to its equatorial axis by observing the oscillations of a cycloid pendulum. Finally, section nine discusses Flamsteed's measurement of a supposed parallax of the pole star and examines the work of James Gregorie and Christian Huygens on the distance of the stars.

5.4.1 Kepler's laws and their application

It is hard to judge Gregory's knowledge and acceptance of Kepler's laws (stated in 2.7) before the appearance of the Principia, simply because of the lack of evidence. In 1684 he had discussed his uncle's solution of Kepler's Problem (see 3.3.3) and we may assume he realised that it was of more than geometrical importance. James Gregorie, David's uncle, had had a copy of the volume of the Transactions containing Mercator's criticisms of Cassini's method of determining a planet's position in an elliptical orbit⁸³. Cassini

⁸³ See Collins to Gregory: 9.6.1670 GTV 101-02.

had assumed in this paper that the empty focus of the ellipse (i.e. that not occupied by the sun) was an equant point, or a point such that the radius vector drawn from it to the planet moved through equal angles in equal times). This assumption is contrary to Kepler's second law, and it was on these grounds that Mercator based his criticism⁸⁴. Curtis Wilson has described this article as delivering 'the coup de mort to the simple elliptic theory', that is, to the theory of elliptical orbits not associated with Kepler's area law⁸⁵. Gregory probably inherited the volume containing Mercator's article from his uncle; his papers still contain a copy of Cassini's original article, and he referred several times in the notes for his Astronomiae to this paper and Mercator's reply⁸⁶, though neither were, in the event, used in the work.

It seems likely that, by the time he began lecturing in 1683, Gregory knew of Kepler's laws from this article. In any case, he would have found them soon in Mercator's Institutiones from which his lectures on astronomy were largely drawn (see 2.7). However, the first three parts of these lectures do not mention the second law, and substitute circular orbits for the first. They give the third law, though, and refer the student to Kepler's Harmonice, Mundi for further details.

As Wilson points out, the first two laws tend, on Kepler's

⁸⁴ PT V (March, 1670) no 57 1168-75.

⁸⁵ Curtis Wilson 'From Kepler's laws, so-called, to universal gravitation: empirical factors' Archive for the history of the exact sciences VI(1970) 89-170 p.129.

⁸⁶ B9; e.g. A56⁶ and Misc. 70.

empirical arguments, to stand or fall together⁸⁷. Before Newton had established these laws as consequences of universal gravitation obeying the inverse square law, the third law was not only more aesthetically satisfying, but on a sounder empirical basis. It would have been a consistent stand-point for Gregory to have accepted the third law, but to have had some mental reservations about the first two. Later he would refer to the ratio of the third law as that 'which Kepler believed and Newton proved'⁸⁸, and he seems to have waited for Newton's 'proof' before accepting the first two laws.

Yet, even after reading the Principia, his acceptance of these laws was not complete. When he wrote part four of his astronomy lectures (probably in 1689) he abandoned the vortex system in which the previous parts had been set and said that 'there is very little doubt' that the planets (as the satellites, in turn, about them) are carried in ellipses about the sun. Even here, though, Gregory did not state the second law, but said instead that it is not quite certain whether the sun lies in one focus of the ellipse or whether the other is an equant point. This latter assumption, he confessed, is perhaps contrary to demonstration, but since it is assumed by astronomers who build tables according to it, he will assume it now. Proposition 16 repeated his doubts of the equant point approximation, but nowhere did Kepler's second law appear. Instead he used the equant devices of Ward and Bouilleau. He also intended that these devices should be used in his Oxford lectures, for in that given

⁸⁷ Wilson op cit(85) p 92.

⁸⁸ Misc. 70.

on 17th November, 1692 he warned his listeners of the geometrical difficulty which would be found in using these.

The introduction to book three of the Astronomiae tackles the matter quite differently. Gregory states that Kepler, in his Astronomia Nova, has shown clearly that only elliptical paths can produce the observed motions of the planets. He explains some of the devices, such as bisection of the eccentricity, to which ancient astronomers had recourse, thus producing near elliptical orbits, with the sun in one focus, out of combinations of circular motions. After Brahe's detailed observations had been made, Kepler determined that the orbits are, in fact, ellipses. Gregory mentions the second law only briefly, in contrast to his full discussion of the ellipticity of the orbits. Not in this introduction, but only in the scholium to proposition 5 is this area law attributed to Kepler.

The first section of this book is devoted to finding the true anomaly from the given mean one. The mean anomaly, the area swept out by a radius vector from planet to sun in a given time is, by Kepler's second law, directly proportional to time. The true anomaly is the angle this radius then contains with the line of apsides, and defines the planet's position in its orbit. If we take the empty focus as an equant point, the mean anomaly is the angle contained by the radius vector joining that focus to the planet and the line of apsides. It was as this angle that Gregory had defined mean anomaly in his astronomy lectures, but in the Astronomiae he defined it rigorously as the area swept out.

First, he explains how the problem may be indirectly solved through tables of corresponding true and mean anomalies, how these may

be built up and used, and then how the rule of false position may be used instead. Proposition 4 gives James Gregorie's solution of Kepler's problem by infinite series, as David had already published it in his Exercitatio. He points out that neither this solution, not Wren's mechanical one, by means of a cycloid, are very convenient for the practical astronomer, and instead he gives some approximating devices. First of these is the simple equant device; viz. allowing the angle at the empty focus to bear the same ratio to 2π as one requires the area swept out at the sun should bear to the entire ellipse (or, equivalently, that the time elapsed bears to the planet's periodictime). The error involved here will, he points out, increase with the eccentricity of the ellipse.

In the following scholium, Gregory gives his opinion on the work of four astronomers: Kepler, Ward, Bouilleau and Pagan. Kepler considered an equant motion about the empty focus, but, realizing it could not agree with the actual behaviour of Mars and, more importantly because it contradicted his hypotheses of the physical causes of planetary motions, he neglected it. Gregory clearly admires him for sticking to the cumbersome and indirect methods involved in the area law and spurning the easy option of an equant device. For Ward, too, who had also held the Savilian Chair of astronomy there is much praise. Ward, while admitting that the sun was the prime cause of the planetary motions, assumed that these motions were so adjusted as to give equable motion about the other focus, and, by skilful geometry, developed theories of all the planets. However, while his method is extremely useful, as a very close approximation, it has not solved the primary problem of resolving the motions exactly according to Kepler's second

law. Over Bouilleau and Pagan, Gregory is far less enthusiastic. He criticizes the former for being unaware that his treatment of planetary motions was equivalent to assuming an equant point at the empty focus, and Pagan since, although his work appeared after the others had published, he did not go so far geometrically as had Ward.

In the next two propositions, though, he shows first how the true anomaly is found from the mean on Ward's hypothesis, and then explains Bouilleau's refinement of this method. Again he warns that this refinement is

'well enough if taken only for a Correction of the
Approximations to the true system as it ought to be'⁸⁹.

Although he refers to Mercator for the judgement that none before Bouilleau had devised a direct system for deriving the true anomaly on the elliptic hypothesis which agreed sufficiently with the observations, Gregory nevertheless strongly criticises Bouilleau for proposing his device as a 'true' system and deriving its physical causes.

These devices are not presented by Gregory as in any sense rivals to Kepler's laws of areas, but, as Mercator had implied⁹⁰, as calculatory tricks approximating to the truth. It seems that his initial study of the Principia alone in Edinburgh in 1687 had persuaded him to rethink his attitude to Kepler's laws, and particularly to the second one, producing the ambivalent references to uncertain points in part four of the Edinburgh astronomy lectures. However, by 1702 (or,

⁸⁹ Ast.(26) 3 1 proposition 7. 392.

⁹⁰ Op cit(84).

rather, by 1697 when he began to write the Astronomiae) the influence of the Newtonian scientists with whom he mixed had persuaded him to accept Kepler's laws as laws of nature, established beyond doubt by observation.

It must be remembered here, however, that the Astronomiae and the astronomy lectures were written for entirely different purposes. The former was a textbook designed to expound the Newtonian cosmology, while the latter were read directly to teenage students who were more interested in calculating a planet's approximate position than in understanding the cosmological principles which underlay its appearance there. This factor alone though, cannot fully account for the different presentation of Kepler's second law in the two works. No new empirical evidence had appeared in the meantime, yet what was merely probable in the Edinburgh lectures was an observational fact in the Astronomiae. The effects of his intercourse with the Newtonian scientists of his day must have been at least a strong contributory factor to this change of emphasis.

5.4.2 The compromise of Cassini's orbit

Then Gregory turns in proposition 8 to Cassini's orbit, which seemed to him at one time to be some kind of compromise between the true and approximate systems. His early awareness of Cassini's observational and theoretical work shows in the 1685 Edinburgh astronomy lectures, where, besides many references to Cassini's observations in part one he also mentions, though disagrees with, Cassini's hypothesis of periodic cometary orbits. It was 1694 when Gregory first heard of Cassini's orbit; during his visit to Newton at Cambridge in May of that year he noted the chief supposed property of the

Cassinian oval, namely, that the areas swept out by radii to one focus from a point moving on the periphery are in the same proportion to the total area of the oval as the angles formed with the major axis of the oval by radii from the same point to the other focus are to 2π ⁹¹. In other words, for a planet moving in this oval with the sun in one focus and obeying Kepler's area law, the empty focus would be an equant point and Ward's approximation would be strictly true. However, the curve is that in which the product of the distances of a point from two foci is constant, i.e. if r and r' are the two distances, $rr' = k$, and, following the analysis of H.W. Turnbull we find that the areas at one focus are not, in fact, proportional to the angles at the other⁹². But neither Gregory nor Newton realised in 1694 that Cassini's claim was false, and Gregory recorded that the use of this curve for a planetary orbit was the reason for Newton's high opinion of Cassini⁹³.

The curve was suggested by Cassini as a planetary orbit in 1693⁹⁴, but it was not until 1699 that Gregory saw a paper concerning it. Jacques Cassini, son of Jean Dominique, deviser of the curve, visited England in spring, 1698, and on 27th, 28th March he met Gregory at Oxford⁹⁵. Gregory had previously resolved to ask him about his

⁹¹ 4.5.1694 C33 RG fo 65 NCIII 441 311-15.

⁹² NCIII 322 n16.

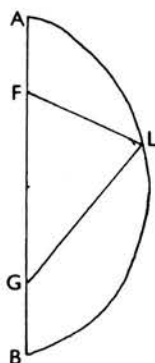
⁹³ Memoranda (91).

⁹⁴ Jean Dominique Cassini 'De l'origine & du progres de l'Astronomie ...' Recueil d'Observations faites ... par Messieurs de l'Academie Royale des Sciences (Paris, 1693).

⁹⁵ Oxford, 12.8.3.1698 A67 RG fo 73.

father's planetary orbit⁹⁶, and Jacques promised when they met to send him a copy from Paris, either printed or in writing. Apparently none of the few fully descriptive printed copies were available, and in January, 1699 Gregory was sent the manuscript copy which is still among his papers⁹⁷.

This paper appears on first sight to use virtually the same analysis as Leibniz had used to show that his harmonic vortex obeyed Kepler's second law (see 5.2.1). Let ALB be half the orbit, F the



focus of mean motion and G the sun, at the other focus. By definition,

$$FL \times GL = c^2 \text{ for some } c.$$

We can imagine the motion about F composed of two components, a radial and a trans-radial one. The radial motion is to and from F along LF and the trans-radial motion is equable about F , in the circumference of a circle radius FL . Since F is the centre of mean motion, the trans-radial speed at L will be as the radius of its circle, that is, as FL . But $FL \propto 1/(GL)$, i.e. trans-radial velocity is reciprocally proportional to distance from G . Therefore, as Kepler had demonstrated in the Epitome, and Leibniz had used in his 'Tentamen', the radius vectors sweep out equal areas about G in equal times⁹⁸.

The fallacy lies in the change from one centre of motion to the

⁹⁶ Paper after A66.

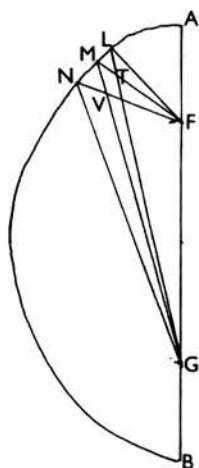
⁹⁷ Q116.

⁹⁸ See Aiton op cit(53) 129.

other. It is not the trans-radial motion about G which is inversely proportional to GL , but that about F . Kepler's demonstration is inapplicable and the proof collapses.

Gregory's reaction to this proof seems to have been, at best, lack of conviction, for he went on to supply his own proof of this property at the end of Cassini's argument. If he accepted the Frenchman's proof he would surely have used it instead of finding his own, and he was certainly skilled enough to detect the flaw in Cassini's work. The reasons why, without accepting its proof as given by Cassini, he still accepted the property itself as at least worth attempting to prove are discussed below in this section.

Meanwhile, Gregory's own alternative to Cassini's proof was not without error.



Let $ALMNB$ be a Cassinian oval, with foci F and G . (i.e. $ALMNB$ is a curve such that for any point L on the curve, $FL \times GL = \text{a constant}$.)

The burden of Gregory's proof lies in showing that if the minimal angles \widehat{LFM} and \widehat{MFN} are equal, so will the areas LGM and MGN be.

The original proof, produced by Gregory on 10th March, 1698/9⁹⁹ contains several claims on the proportionality of lines and curves which are unjustified unless angles \widehat{LTM} and \widehat{MVN} are equal. It then introduces the claim that the smallness of the two angles implies

⁹⁹ C116.

that FL, FM and FN can be regarded as parallel, as can GL, GM and GN, and that LMN can be taken as a straight line. Clearly, on this supposition, angles LTM and MVN are equal, but the original claims (that, for example, the arcs of radius FL, FM, terminated by FM, FN are proportional to LT, MV) are based not on this, but on the vague justification that the angles LFM and MFN are very small. The proof eventually published in the Astronomiae¹⁰⁰ avoids this by presupposing the parallelism of the two triples of lines and the identity of LMN with a straight line, again on the basis of the smallness of the angles LFM and MFN. However, although this approach shows some improvement over the earlier one, it is still unjustifiable. Indeed, the counter proof of Halley and de Moivre was based on the very fact that the Cassinian oval is not a straight line.

As so often in his mathematical work, Gregory allowed himself to be satisfied with an argument whose only virtue was that it appeared to prove a result of whose truth he was already convinced. In the case of the catenary for example, he attempted to prove results obtained by Leibniz and the Bernoullis; for Cassini's oval he had not only the assurance of the two Cassinis, but that casual remark of Newton's made nearly five years earlier¹⁰¹, on the interesting property of Cassini's new curve. However, we have seen that Cassini's argument was unlikely to have convinced him, and surely one casual comment, even if dropped by Newton, was not enough.

Gregory was pleased with his proof. He mentions it twice in the

¹⁰⁰ Ast (02) 3 1 proposition 8 217-18 (But not in all copies, see infra)

¹⁰¹ 4.5.1694 C33 RG fo 65 NCIII 441 311-15.

notes for his Astronomiae saying 'Cassini's Orbita is excellent. It erres not in the angles as I have demonstrated'¹⁰². There is no evidence as to what prompted him to introduce the parallelism and linearity at the beginning rather than the middle of his proof, but it is quite clear that in spite of this alteration he felt no qualms about including it in his book. Nor did Arbuthnot, nor, more surprisingly, Keill, detect any error in the argument when they read the work in manuscript, probably because they, too, were predisposed to accept the result¹⁰³. A hint from Keill, however, could have explained the alteration in proof structure.

But why were these men, especially Gregory, with Cassini's arguments before him, predisposed towards this result? The answer seems to lie partly in the inherent attractiveness of a solution to the planetary motions which promised to combine Ward's facility in operation with Kepler's accuracy in description. Once this solution had been promised, it is easy to understand why Gregory would look for an alternative proof rather than reject it out of hand when he found Cassini's own line of argument unconvincing. Less admirable, but equally human, is Gregory's easy satisfaction with his own most unconvincing proof. The explanation might also lie in the personalities involved. Gregory was friendly with Cassini fils, with whom he corresponded at least once about his father's work after meeting him at Oxford¹⁰⁴. More importantly, Huygens was a friend of Cassini whom he had mentioned frequently to Gregory when the latter visited him in

¹⁰² Misc. 70.

¹⁰³ Misc. 7.

¹⁰⁴ J.D. Forbes Autograph Collection no 42 SUL.

1693¹⁰⁵. Gregory was prepared to attack Flamsteed's supposed measurment of the parallax of the pole star, a result whose apparent attraction is, if anything, greater than that of the Cassinian oval, when his relationship with Flamsteed was at a low ebb (see infra). He was not, though, prepared to attack the planetary orbit whose description had been sent to him as an act of friendship, and which had been developed by a friend of Huygens, a man for whom he felt great academic respect and personal loyalty.

The discussion in the Astronomiae of the oval lends itself better to the second argument. Here Gregory states quite definitely that this ingeniously contrived orbit links Ward's hypothesis and Kepler's second law, but cannot possibly have any physical reality. While it is a fair approximation for small eccentricities, it is, in fact, less accurate than a circle as the eccentricity becomes larger. Also, it is contrary to the law of a natural centripetal force, which, like any virtue propogated rectilinearly from a point source obeys the inverse square law, under which force such an oval could not be described. As such it is to be rejected from astronomy.

The force of these views may have originated with Newton, however. In May, 1701, Gregory noted his intention

'to ask Mr Newton about Cassini's figure of a Planets Orbit: of its reconciling Ward and Kepler's hypotheses'¹⁰⁶.

¹⁰⁵ See 4.1.

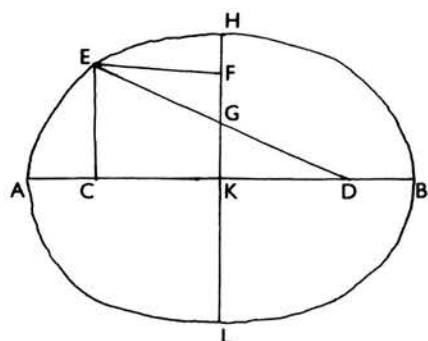
¹⁰⁶ A68².

Unfortunately the orbit is not mentioned in the notes of his meeting with Newton the following June and one cannot judge precisely Newton's influence on the final presentation¹⁰⁷. It is scarcely possible that Gregory produced his proof of the Cassinian property for Newton's approval before publication, nor does the memorandum imply that he had this intention. It appears likely, however, that he discussed with Newton the use of the curve as a planetary orbit and the extent to which a curve satisfying Ward's hypothesis and Kepler's second law, but contradicting Kepler's first law, could be said to reconcile the two hypotheses. If this was the case, the rejection of the curve as a feasible planetary orbit might have been something which Gregory only accepted reluctantly after the discussions with Newton. In any case, his memorandum shows that even at a very late stage in the composition of the Astronomiae he was still concerned with the curve and the possibility of its use as a planetary orbit.

The next stage came two years after the publication of the Astronomiae in the summer of 1704. Halley and De Moivre had given Gregory an incontrovertible proof that in any curve symmetrical about two axes and everywhere concave towards the centre, the angles formed at one point on the axis by the radius joining it to a point on the circumference could not be proportional to the areas swept out by radii from the same point on the circumference to the corresponding point on the other half of the axis¹⁰⁸;

¹⁰⁷ RG fo 79.

¹⁰⁸ E126.



Let AHBL be a curve everywhere concave towards K, symmetrical about AB and HL, with C and D two points on AB such that $CK = KD$.

Suppose that for any point X on the periphery of the curve (X not

marked in figure)

$$\widehat{XCA}: 2\pi = \text{area XDA}: \text{area AHBL}.$$

Take E, such that $\widehat{ECA} = \pi/2$. Join ED, and drop EF perpendicular to HL.

$$\text{But } ECA = \pi/2 \Rightarrow \text{area EDA}: \text{area AHBL} = \pi/2: 2\pi = 1: 4$$

$$\Rightarrow \text{area EDA} = \frac{1}{4} \text{ area AHBL}.$$

Also, since $EDA + EHBD = \frac{1}{2} \text{ AHBL}$,

$$\text{area EHBD} = \frac{1}{4} \text{ area AHBL} = \text{area HKB}.$$

Subtracting HGDB, $EHG = \Delta GKD$.

But $\Delta EFG = \Delta GKD \Rightarrow EHG = \Delta EFG$, which contradicts the supposition that AHB is everywhere concave towards K.

This underlines clearly the basic flaw in Gregory's argument, that is, the assumption of linearity, and proves his proposition 8, book 3, on the Cassinian orbit to be in error. Gregory noted on 2nd August, 1704 that, besides the intended changes to scholium proposition 66, book 2 on refraction which were never made, it was essential to alter the proposition on the Cassinian orbit¹⁰⁹. His first intention was to omit all but a description of the curve's construction and to notify the learned world of this in a letter to

¹⁰⁹ E127 .

Sloane for the Transactions¹¹⁰, but by 22nd August he had devised at least a partial answer to Halley's argument; the curve is not necessarily always concave towards the centre¹¹¹. For differing ratios of the minor axis to the distance between the foci the curve takes on various forms including a dumb-bell shape which is convex towards the centre around the minor axes. In such a case, Gregory still believes, since EF does not lie wholly inside the curve, and Halley's argument is inapplicable it would be possible to find a Cassinian orbit in which the angles at one focus were as the areas at the other. However, he does not follow this up and has perhaps recognized the flaws in his original argument. Instead he describes at great length the various forms which the oval can take and the conditions under which it does so. During August and September he developed these (basically elementary) properties from the result that $HL^2 = AB^2 - 2CD^2$, applying fluxions to the discovery of maxima and minima, in which use of infinitesimals he was quite at home. He then summarized the results in an article for the Transactions.

Gregory first intended that this paper, with his comments on the curve, should contain a specific admission of his error in the Astronomiae. He intended to leave anything more (presumably a more detailed discussion of the question of the proportionality of angles and areas) for an answer to any comments on the paper. This memorandum concludes enigmatically

¹¹⁰ E128.

¹¹¹ E129.

'Si non, I must do my best by annoying my enemys'¹¹².

and one is tempted to wonder whether Halley was one of the 'enemys' of whom Gregory was thinking. Although the counter proof was originally introduced as the argument of Halley and De Moivre, it is thereafter referred to by Gregory as Halley's alone. Gregory heard on 21st August from John Keill that, according to Cheyne, Halley intended to write against his errors as well as Varignon's. He dismissed this as 'spiteful stuff', but it seems likely that relations between them were somewhat strained at this time¹¹³. It might also be relevant to note here that it was through Halley that Hearne heard a year later of the error in Gregory's Astronomiae and his attempts to correct it secretly¹¹⁴.

In the original draft of his article for the Transactions, Gregory does not mention Halley or De Moivre. He does, though, admit that, on rereading his Astronomiae, he discovered that he had been in error over Cassini's oval. He plays down the fault, claiming that it is of no importance to astronomers as it leads to nothing further and was for other reasons categorized as a curve to be rejected from astronomy. However, as a sop to the geometers, whom alone this error concerned, he will give the properties of the curve which he has since discovered¹¹⁵.

¹¹² E128 Hiscock 19.

¹¹³ E128 Hiscock 19.

¹¹⁴ Thomas Hearne Remarks and Collections vol 1 (1705-07) ed. C.E. Doble, Oxford Historical Society vol 2 (Oxford, 1885) 13.

¹¹⁵ The exact dates on which Gregory wrote these papers are hard to determine. Among his papers there are two versions; a first draft (misc. 48) dated 10th September, and another, apparently a later draft, but dated August, 1704 (A28).

However, in October, 1704 Gregory was in London and discussed his problem with Newton who, he noted on the 22nd

'would have me bring in my apology by the by in the middle of the paper about Cassini's Orbita, and not at the beginning or ending of it'¹¹⁶.

This is the plan Gregory eventually adopted, but 'apology' seems a generous description of the brief mention he makes of his error. The published paper adds his newly discovered properties of the curve to the reasons in the Astronomiae for which the curve is unacceptable as a planetary orbit¹¹⁷. As he has shown that the extreme cases are clearly unacceptable (the orbit splits into two separate closed curves), then by a principle familiar to geometers, the middle range of cases cannot be acceptable either. He adds in passing that a planet on this curve will not describe angles at one focus proportional to areas at the other 'as I recently wrongly believed'¹¹⁸. Gregory had been understandably reluctant to publicly confess his error, but had originally been prepared for a full, if grudging, admission. This final concession of a few words in the middle of an article was apparently adopted on Newton's advice, and one can only suppose that Newton's reluctance to allow his disciple to acknowledge an error in his handling of infinitesimals was even greater than Gregory's own reluctance to do so.

¹¹⁶ E127 Hiscock 20.

¹¹⁷ PT 24 (September, October, 1704) no 293 1704-06. Gregory's manuscript A28 dated October, 1704.

¹¹⁸ 'ut perperam nuper sentiebam' ibid 1706.

However, Gregory's book had been in the hands of the scientific world now for over two years, and the paper in the Transactions would not erase the fault in it. On 24th July, 1705, Hearne reported a tale he had heard from Halley; the latter had pointed out a great mistake in Gregory's Astronomiae 'notwithstanding it was the most part taken from Sir Isaac Newton's Book'. Gregory had then reprinted the sheet involved and replaced it in as many copies as he had left or could get secret access to. All this was done with no acknowledgement to Halley¹¹⁹.

Hearne's tale, although its manner of presentation is tinged with his bitterness against the Scottish group at Oxford, is in its essentials true. At least two copies of the 1702 edition have been so altered¹²⁰. These copies are in the Bodleian Library, Oxford, and were presumably among those Gregory could most easily get hold of. Pages 216-19, on which proposition 8, section 1 of book 3 describes the Cassinian orbit, give, in these copies, a very different account from the original. Here, after the same geometrical description of the curve, Gregory's 'proof' of the equant property is replaced by a proof, differing only trivially from that of Halley and De Moivre, that the angles at one focus cannot be proportional to the areas at the other.

In 1715, a second Latin edition appeared at Geneva. Pages 327-29 of this work contained the altered insertion of the two Bodleian copies, and this was followed on pages 330-33 by Gregory's

¹¹⁹ Hearne op cit(114) 13.

¹²⁰ Bod. D.6.12.Art. and Savile B13.

article on the orbit from the Transactions. Stone's second English edition of 1726 (the first English edition also appeared in 1715) followed the format of the Latin 1715 edition in this proposition, except that it did not refer to the previous publication of Gregory's comments on the orbit in the Transactions. Since this alteration was by no means in all copies (EUL, NLS, BM and RS all have only unaltered copies) Gregory's posthumous editors were either very lucky in their choice of source, or knew of his error and his attempts to rectify it. However, apart from Hearne, I have found no mention of this by any of Gregory's contemporaries.

In sum, then, Gregory was first drawn to Cassini's orbit by the promise it held of a compromise between Kepler's physically satisfactory area law and ~~Word~~'s geometrically satisfactory equant hypothesis. Even when he realised that Cassini's proof of the curve's supposed property was erroneous, Gregory was reluctant to abandon it. Instead, he produced his own proof, but his shaky grasp of infinitesimals let him down, and his proof was erroneous. However, even after Newton had convinced him (or so seems likely), that the curve was physically unacceptable, Gregory was proud enough of his proof to publish the curve for its sake. Halley and De Moivre soon revealed his error, and Gregory was forced to retract. Newton persuaded him to play down his apology, and there seems to have been little comment on the matter. Yet for Gregory, who wished to see himself as the interpreter of Newtonian mathematics to the world, this public embarrassment over the analysis of infinitesimals must have been a bitter pill to swallow.

5.4.3. Flamsteed's measurements of parallax

As we saw in 5.4.2, the personalities involved may have helped

persuade Gregory to accept Cassini's orbit. His treatment of Flamsteed's observations ~~were~~^{was} even more clearly a case where his personal feelings influenced his scientific judgement. Here, with Newton's backing, he rejected the work of Flamsteed (with whom he was then on very bad terms) on very slender reasons. Instead he adopted the work of Christian Huygens and his uncle, James Gregorie, two of the earliest influences on his scientific thought.

If the earth is moving through space, the distances of the stars from us and the apparent relationships between them should alter in an annual cycle. However, the stars are so immensely distant as compared with the earth's orbit, that these effects are almost insensible. The detection of such an effect would have implied that the earth indeed moves and have strongly supported the Copernican system.

James Gregorie had considered the problems of stellar distance and the radius of the earth's orbit on several occasions. Proposition 87 of the Appendix to his Optica Promota outlined a method, resurrected later by Halley, for discovering the sun's distance by two simultaneous observations of the transit of Venus ^{or} ~~of~~ Mercury across the sun's disc. In the Geometriae Pars Universalis he deduced that this distance is insignificant compared to the distance of the fixed stars, and employed a method which compared the amount of sunlight reaching us when reflected off a planet with the light reaching us directly from a star to discover that star's distance. When these lights appear the same, by knowing the proportion of sunlight lost in reflection we may calculate the relative distances of sun and star. Finally, only months before his death, James wrote

to Oldenburg¹²¹ sending his ideas on proving the earth's motion by an observation of parallax, prompted to this by reading Hooke's tract which discussed the possibilities of doing so by observing stars near the zenith¹²². Gregorie's method is to measure the angular distance between two stars from diametrically opposite points of the earth's orbit. Any difference between these observations can only be a parallax effect arising from the earth's annual motion, which would therefore be proven by the detection of any such difference. (The points of the earth's orbit should be so chosen as to be coplanar with the sun, and with the stars to be measured,) David had access to all these when preparing his Astronomiae: the first two in published form and the third as a draft of the method sent to Oldenburg by James, which is still among David's papers¹²³.

With Huygens, Gregory had discussed the parallax of the earth's orbit during his visit to Holland in 1693, and he had even copied down Huygens' argument for believing this parallax to be insensible¹²⁴. In this argument, Huygens deduced that the angle subtended at a star by the radius of the earth's orbit would be only 100 times greater than the angle subtended by that star at the earth. But the star's diameter is insensible to us, even through a telescope which magnifies a hundredfold, and therefore the diameter of the earth's orbit is likewise insensible in comparison to the distance of the

¹²¹ Gregory to Oldenburg: 8.6.1675 GTV 306.

¹²² Robert Hooke On attempt to prove the Motion of the Earth (London, 1674).

¹²³ C142.

¹²⁴ June, 1693 A15 NCIII 417 272-75.

star. While writing the Astronomiae, Gregory discussed this argument again with Newton, who appears to have supported it¹²⁵. Huygens' Cosmotheoros which appeared in 1698 gave another method of detecting the distance of the fixed stars based on the intensity of their illumination. In this case the sun's light should be diminished by lenses or similar means until its intensity is the same as that of a fixed star. The sun's image will appear insensible, but its ratio to the sun's diameter may be found by calculation and thus the ratio of the sun's distance to the star's distance from the earth may be found.

In book 3, section 6 (which deals with the diameters of the planetary orbits) and section 9 (on the distance of fixed stars) are composed almost entirely from this material. However, section 9 contains one more important component; the argument against Flamsteed's claim to have measured the parallax of the pole star. A letter of Flamsteed's to Newton in February, 1695 refers to the parallax he believed he had detected in the pole star¹²⁶, but Gregory seems to have first heard of it from Caswell 3 years later. In March, 1698, he noted that Caswell had told him of such observations, but even then he had no details of them¹²⁷. The following summer of 1698 Gregory was in London, and he discovered more of these measurements and probably heard them discussed by his scientific friends in the capital. Now he noted of Flamsteed

125 London, June, 1698 A79 RG fo 62.

126 Flamsteed to Newton: 7.2.1695 NCIV 493 83.

127 24.3.1698 E90 Hiscock 9.

'It does not appear to me that his observations evence
a Parallaxe in the great orbe'.

Later he added smugly to this that

'Mr Newton says that my Uncles methode of determining
the parallaxe of the great orbe is the best possible'¹²⁸.

However, Wallis' reaction was quite different. He also heard
from Caswell of Flamsteed's discovery, and he wrote to him in
August, 1698, asking that he might publish the results in the forth-
coming volume of his Opera. He told Flamsteed,

'The thing will be an honour to you, and to our nation'¹²⁹.

The letter in which Flamsteed sent his results was the ill-fated one
containing references to his provision of observations to Newton.
This led to a breach between Newton and Flamsteed, and to Flamsteed's
bitterness against Gregory for his 'officious flattery' (see 1.9).
Under these circumstances, Gregory's attack on the contents of the
letter seems at best to have been tactless and impolitic. Indeed,
the conviction, backed by Newton, that his uncle's method of measure-
ment was better than Flamsteed's appears to have preceded the
detection of any flaw in the latter. As well as the comments quoted
above, he also remarked in his notes for the Astronomiae that the only
way to detect parallax in the earth's orbit was by differences in the

¹²⁸ Londone, June, 1698 A79 RG fo 62.

¹²⁹ Wallis to Flamsteed: 13.8.1698 NCIV 590 278.

distance between two stars (that is, by James Gregorie's method), and that

'Mr Flamsteed's from different distances of the pole starr from the pole is naught'¹³⁰.

Only later, though, could he find any other way in which Flamsteed's results might have arisen when he noted that they might result from a greater weight, or a greater cold, at the south pole¹³¹.

What criticism did these enigmatic references to greater weight or cold conceal? Flamsteed's conclusions were based on the assumption that (disregarding the moon's attraction on the earth's spheriodal shape, which does not affect these results) the earth's axis remains parallel to itself, for he was, in effect, comparing the direction of the pole star to that of the earth's axis extended. Clearly a change in their angular separation while the latter is constant implies a change in the former which is most readily attributed to a change in the earth's position. However, the same result arises from a change in the latter direction, and Gregory's argument was based on this possibility. (The results were, in fact, mainly the result of annual stellar observation, as Bradley showed in 1728¹³².)

However, the causes which Gregory suggested for such a change in direction of the earth's axis were unconvincing. It might arise

¹³⁰ Misc. 70.

¹³¹ Ibid.

¹³² PT XXXV (December, 1728) no 406 637-61.

he conjectured, from an unequal solar attraction on the northern and southern hemispheres, caused by a greater density (because of greater cold, an uneven distribution of land mass, or some other, unknown, cause) around the south pole. But, even if there were such an uneven distribution of density, which from the known disposition of land masses, seemed unlikely, it would not, as Flamsteed pointed out to Caswell¹³³, cause such a nutation, but merely a change in the earth's centre of gravity.

It was not until after the publication of the Astronomiae that Gregory saw the paper of Jacques Cassini, which effectively destroyed Flamsteed's arguments¹³⁴. This paper ~~had~~ appeared in the Memoirs of the French Académie ^{for (published in 1702)} in 1699/and, after considering Flamsteed's observations, showed that they were, in fact, contrary to the appearances to which the earth's annual orbit should give rise¹³⁵. On 4th November, 1702, Gregory noted that he had transcribed this paper and that Flamsteed 'made a great Bustle about it' but confessed that Cassini~~s~~ was in the right¹³⁶. (The next month, though, Flamsteed wrote to Sharp that he had written a paper answering Cassini's objections¹³⁷.) Two years later, Halley told Gregory that he, too, had a paper ready to print, refuting Flamsteed's measurements¹³⁸.

¹³³ Flamsteed to Caswell: 5.9.1702 Baily op cit(12) 205-08.

¹³⁴ 23.10.1702, E102.

[Paris 1702]

¹³⁵ Mémoires^e de l'Académie Royale des Sciences 1699/247-53.

¹³⁶ E106.

¹³⁷ Flamsteed to Sharp: 14.12.1702 Baily op cit(12) 209-10. The answer is Flamsteed to Wren: 17.11.1702 Royal Greenwich Observatory MS P.R.O. 33 ff 164^v - 168^r.

¹³⁸ 16.10.1704 E113.

However, this paper does not appear to have been published, and Gregory's memorandum gives no clue of the arguments Halley had intended to use.

Gregory's attack on Flamsteed's paper had been two-pronged. On the one hand, Flamsteed's observations could be explained by a change in the direction of the earth's axis, and the only sure way to detect parallax was by James Gregorie's method; on the other, Huygens' argument showed that Flamsteed's method could not give sensible results and one could only estimate the distance of the fixed stars by the indirect methods of Huygens and Gregorie which depend on the relative intensities of illumination of sun and star.

Flamsteed's letter to Caswell, which discussed Gregory's Astronomiae, and, as we have seen above, criticized his suggested reasons for the change in direction of the earth's axis, had a fairly wide circulation¹³⁹. Caswell showed it to Gregory, who made a summary of it which, as Hiscock puts it 'softens nothing'¹⁴⁰. The letter also attacked the other line of argument Gregory had used. The insensibility of the earth's orbit depended on Huygens' observation that the star's diameters were insensible even under a magnification of one hundred, and Flamsteed did not accept this. He referred to his own observations of stellar diameters and claimed that Huygens' results arose from his use of a smoked objective lens

¹³⁹ Flamsteed to Caswell: 5.9.1702 Baily op cit(12) 205-08; Flamsteed offered to send Sharp a copy, (Flamsteed to Sharp: 14.12.1702 ibid 209-10) and sent a copy to Thornton (Flamsteed to Thornton: 18.2.1703 ibid 747-48). As well as Gregory, Caswell showed his copy to Wallis (Flamsteed to Wallis: 10.10.1702 ibid 208-09).

¹⁴⁰ E105 Hiscock 12-3.

in his telescope. As for James Gregorie's proposed method of measuring parallax, Flamsteed pointed out to Wallis that this was, by its nature, more liable to error than continued measurements of one star by a fixed instrument, such as he had made of the pole star¹⁴¹.

Yet Gregory had Newton's backing for these contentions. Newton had told him that James Gregorie's method of measuring parallax was the best and had discussed, and apparently accepted, Huygens' argument for the insensibility of the effect produced by the earth's motion. Gregory's eagerness - perhaps not instigated, but certainly encouraged, by Newton - to rebut Flamsteed's apparent triumph of observation led him into an untenable position. He could only produce comparatively weak arguments against those observations, and he found himself arguing with Flamsteed on the latter's ground: the techniques and practice of observational astronomy. Nor were Gregory's arguments universally well received. Flamsteed's letter to Wallis of 10th October, 1702, strongly implies that the latter had been neither pleased nor convinced by them¹⁴². Whiston was to call them

'this evasion of Dr Gregory's ... [which] ... is not small error of his and leaves a blemish upon a work otherwise valuable for demonstrations strictly geometrical'¹⁴³.

¹⁴¹ Flamsteed to Wallis: 10.10.1702 Baily op cit(12) 208-09. (This point is not made in Flamsteed to Caswell: 5.9.1702 ibid 205-08).

¹⁴² Flamsteed to Wallis: 10.10.1702 ibid 208-09.

¹⁴³ William Whiston Sir Isaac Newton's mathematical philosophy more easily demonstrated (London, 1716) 238. (First appeared in Latin, 1710 and consisted of lectures given between 1704 and 1708).

Gregory's act in copying out Cassini's long article in his own hand, an article of which he made no further use, suggests a stronger interest in the acceptance or otherwise of Flamsteed's results than an impersonal striving for objective truth¹⁴⁴. It may have been intended for a second edition of the Astronomiae, but November, 1702 seems a little early to have been making full transcriptions for such an end. Indeed, under the circumstances, Cassini's paper may well have appeared to Gregory as a personal vindication. The arguments he himself had used against Flamsteed might be weak and easily refuted, but it was now shown that the position he had taken on the matter had been fully justified.

¹⁴⁴ 4.11.1702 E106 notes he has made this copy, now SUL MS 31,010 ff 110,111.

5.5 Book four: the astronomy of the satellites

This book discusses the theories of the "secondary planets" or satellites, particularly that of the moon. Gregory explains in the preface that the combination of forces acting on a satellite makes its orbit extremely complicated, and gives it

'such Unequalities as the Astronomers hop'd to account for by their Hypotheses, sooner than the Philosophers cou'd explain them from Physical Causes, till Sir Isaac Newton was so happy as to do it'¹⁴⁵.

The first four sections deal generally, and mainly qualitatively, with the effects which the gravitational pull of the sun has on the orbit of a satellite about its primary. The results here are based for the most part on the corollaries to proposition 66, section XI, book 1 of Newton's Principia. Gregory then moves on to apply these to the moon, and in section 6, after discussing the lunar tables of Tycho, Kepler and Horrox, he introduces Newton's theory of the moon. He next considers eclipses of the sun and moon before continuing to the satellites of other planets, their axial rotation, shape, magnitude and density, with Huygens' observations of Saturn's ring. In the twelfth section he gives a brief account of the moon's effect on our tides. Here in one of his surprisingly rare explicit references to Newton, Gregory points out that Kepler was the first to perceive the causal rôle of the moon, but Newton, acting on this hint, so enlarged the theory as to make it his own.

¹⁴⁵ Ast (26) 4 preface 466.

5.5.1 Newton' lunar theory

In the spring of 1700, Gregory had visited Newton in London, and had made a copy of his theory of the moon¹⁴⁶. Its publication in the Astronomiae was the culmination of Gregory's interest in Newton's lunar theory. He was soon aware of its debt to Flamsteed's tables and observations, but his early admiration of this work soon became criticism as his relationship with Flamsteed deteriorated. In spite of the lack of Newtonian lunar tables, this theory was published by Gregory (and later by Whiston) as a virtually complete theory. Flamsteed, to whom the theory had been promised, was furious. Perhaps Halley and Newton, suspecting that this would be his reaction, dissuaded Gregory from letting him see the Astronomiae before publication. Certainly Gregory's desire to know Flamsteed's opinion of the work suggests that he was unaware of Newton's broken promise about the lunar theory.

Gregory's enthusiasm for Newton's new theories had early centred on their application to the moon, and his certainty that an analysis based on the principles of universal gravitation would eventually render up a precise account of its motions. Thus in his Edinburgh Astronomy lectures he regrets that he cannot use Newton's theories as there are no tables for them, but it is the only natural and probable theory of the moon, and there is no doubt that tables will soon be produced. The moon's librations, too, had been explained by Newton's elegant hypothesis. At Gregory's first meeting

¹⁴⁶ C121² RG fo 15 is dated 28.2.1700, but this is probably the date of composition rather than of Gregory's copy. His notes below the theory are dated 25.3.1700.

with Flamsteed in 1691 one of the topics they discussed was Newton's lunar theory, and its agreement with observation¹⁴⁷. In autumn, 1693, Gregory examined Newton's lunar theories in great detail, while he wrote the Notae to book 3 of the Principia. Here he was very much concerned with exhibiting the agreement between Newton's computed results and Flamsteed's 1680 tables¹⁴⁸. These tables apparently retained the Horroccian theory for minimum eccentricity, but used a scheme equivalent to Kepler's area law¹⁴⁹ for greater eccentricities, thus achieving an accuracy far in advance of anything previously available. However, as Gingerich and Welther point out, this accuracy is not incontrovertible evidence that Flamsteed was using a direct method of areas¹⁴⁹. It is certain, though, that Flamsteed was employing a version of Horrox's theory for the basic structure of the tables.

Gregory refers to these tables, which appeared seven years before the Principia, as being built on Newton's physical system, as appears from their elements¹⁵⁰. Nine years later, Gregory was to refer scathingly to Flamsteed calling Newton's theory of the moon 'the Horroccian Theory of the Moon Corrected by Mr Newton'¹⁵¹, but it

¹⁴⁷ Gregory to Newton: 27.8.1691 NCIII 370 165-66.

¹⁴⁸ John Flamsteed 'Doctrine of the sphere' published in Jonas Moore A new systeme of mathematicks (London, 1681). The tables are on pp 95-97.

¹⁴⁹ Owen Gingerich and Barbara Welther 'Note on Flamsteed's Lunar Tables' British Journal for the History of science 7 (November, 1974) 257-58.

¹⁵⁰ Notae 134.

¹⁵¹ 28.9.1702 E105, Hiscock 13.

seems that in the Notae (written when he was out of favour with Newton, and judging by the written work alone) he freely acknowledged their similarity. However, he also lamented the neglect of Kepler's principles by other modern compilers of tables¹⁵², and he may merely have meant that both Newton and Flamsteed worked with a due regard for Kepler's laws. In conflict between the two, however, it was already clear in 1693 where Gregory's sympathies lay. In the scholium to proposition 35, book 3 of the Principia, Newton had suggested that the differences between his computations and tables derived from observation might lie in errors of observation. Gregory noted that this is certainly the case, since neither Flamsteed nor any before him built their tables on the true principles¹⁵³.

Flamsteed's lunar tables are mentioned several times by Gregory in the next few years. In May, 1694 he noted from Newton himself the continued use he had made of Flamsteed's tables¹⁵⁴ and in September of the same year he noted the numbers of lunar observations which Flamsteed had given or promised to Newton¹⁵⁵. But in 1698 he noted that Flamsteed's irascibility was causing him to hold back his observations and delay Newton's theory of the moon¹⁵⁶. It was possibly at the same time that Newton told him of Flamsteed's supposed plagiarism of his lunar tables, which story Gregory duly

¹⁵² Notae 135.

¹⁵³ Notae 162.

¹⁵⁴ C33 RG fo 65 NCIII 441 311-22, p.313.

¹⁵⁵ 1.9.1694 RG fo 26 NCIV 468 7.

¹⁵⁶ C62.

inserted in his Notae¹⁵⁷.

In the Notae of 1693, Gregory's comments on Flamsteed's lunar tables had been most complimentary, and he underlined their importance as observational confirmation of Newton's physical principles¹⁵⁸. However, as explained in chapter 1.9, Newton's relationship with Flamsteed (and consequently Gregory's too) steadily deteriorated. As a consequence, these admiring comments on Flamsteed's tables could not be allowed to stand. Gregory returned to them to remark that in December, 1698, Newton had informed him that these tables were not in fact, Flamsteed's, but had been stolen from Halley¹⁵⁹. This seems unlikely, in spite of Newton's contention, as reported by Gregory, that he had seen Halley's autograph version of the tables, but the implications for Gregory and the tenor of the Notae are clear. These admirable tables, built on the best principles and serving to confirm the Newtonian hypothesis, were not the product of Flamsteed (from whom Gregory, as in the matter of stellar parallax, wished to detract all possible glory) but of Edmond Halley, and thus they might retain the important rôle in which Gregory had placed them.

For the Astronomiae, Gregory set out to give an account of Newton's lunar theory as presented in the first edition of the Principia with a critical examination of the lunar tables so far

¹⁵⁷ Notae 162.

¹⁵⁸ Notae to Principia book 3, passim.

¹⁵⁹ Notae 162

produced, and had begun to do this by the spring of 1699¹⁶⁰. In May, 1699 he noted that his account of the moon was completed but it was only in the spring of 1700 that he was able to make his own copy of Newton's recently written new theory of the moon¹⁶¹.

In section 6 of book 4, Gregory considers lunar tables, their computation and use, pointing out that we must always be ready to construct new tables, adapted to newly discovered errors in the old theory. He also briefly mentions the omissions or confusion of several separate inequalities in the tables of Tycho, Kepler and Horrox. Then Newton's theory of the moon is put down. As he had in 1693, though not so generously, Gregory mentions the use Newton has made of Flamsteed's observations, although the paper which he had copied from Newton makes no such gesture. (The translation of 'plurima' as merely 'several' to describe the number of the moon's places given to Newton by Flamsteed makes this acknowledgement appear more niggardly in the English editions than Gregory had originally intended.) Again, as in 1693 and as in his notes of 25th March, 1700 made on his copy of Newton's theory, Gregory attributes any discrepancies between observation and Newton's computed places to an uncertainty in observation. Yet, immediately below this, on his copy of Newton's theory, Gregory had noted that when concerned with the arrangement of tables it is sometimes necessary to draw back from the true philosophy. This suggests that he did appreciate the ad hoc nature of the devices used by Newton to display the inequalities

¹⁶⁰ Misc. 70.

¹⁶¹ Ibid, Cl2l² RG fo 15.

whose existence, but not size, he had used his gravitational theories to deduce. However, in a choice between Newton's physical arguments and Flamsteed's observational evidence, Gregory's natural bias towards theoretical rather than practical astronomy, quite apart from any considerations of personality, would persuade him to uphold the former.

There was still, though, no sign of tables to accompany Newton's theory; a few more inequalities had been detected, giving seven in all, but only the limits within which they varied were given. Gregory was still in the position in which he had found himself when giving his Astronomy lectures at Edinburgh. He was still telling his readers to use the theories on which tables had been built; the only difference being that now he was able to give more details of the alternative Newtonian theory which would one day be available. This was also the plan followed by William Whiston in his Astronomy lectures, first published in 1707¹⁶². In lecture 11, first given on 27th October, 1701, Whiston regretted that he had to be content for the moment with Horrox's lunar hypothesis, since although Newton had discovered the true cause of the lunar inequalities he had not yet established an entire theory of the moon a priori, nor computed tables. Yet in lectures 30 and 31, given on 29th November and 6th December, 1703, after Gregory's Astronomiae had made it available to him, Whiston gave Newton's theory of the moon in full, with his own running commentary interspersed, introducing it as the theory of which he had despaired when giving Horrox's substitute. Indeed, both

¹⁶² William Whiston Praelectiones Astronomiae Cantabrigiae in scholis publicis habitae (London, 1707).

Whiston and Gregory regard this theory, if not as a complete a priori analysis of the moon's motions, at least as one which need only have the corresponding tables computed to replace all previous tables and consistently predict the moon's position to within the limits of observational error.

Newton was probably not so happy with the state of his lunar theory. He complained to Halley that the problem of the moon made his head ache, and in later life told Conduitt of his intention to attempt the moon again, should he live long enough¹⁶³. Francis Baily has pointed out, however¹⁶⁴, that these researches never were resumed, and the paper published by Gregory was essentially his last word on the problem. Jean Sylvain Bailly describes the problem as 'celle où Newton s'est enveloppé de plus d'obscurité' and suggests that he had not entirely grasped the complexities of the problem. He regarded Newton's theory only as an adaption of Halley's improvement to Horrox's theory¹⁶⁵. This was also Flamsteed's view. He was not only unimpressed by Gregory's claims for the accuracy of the theory, but was also bitterly resentful that the theory had been given to Gregory and Halley before himself.

¹⁶³ David Brewster Memoirs of Sir Isaac Newton 2 vols (New York and London, 1965) reprint of Edinburgh, 1855 edition ii 157, 8.

¹⁶⁴ Bailly op cit(12) 706.

¹⁶⁵ Jean Sylvain Bailly Histoire de l'astronomie moderne 3 vols (Paris, 1779) ii 508.

When asking Flamsteed for the lunar observations necessary for the perfection of his theories, Newton had promised that Flamsteed would be the first to receive the completed theory, and that it would never be published without due acknowledgement to Flamsteed¹⁶⁶. These promises had manifestly not been kept.

Flamsteed wrote to Lowthorp on 10th May, 1700, describing an elliptical conversation he had had with Newton on the fate of the lunar theory. Flamsteed knew that Newton had imparted his results to Gregory and to Halley, but remained deaf to Newton's hints that he, too, had only to ask for a copy. Flamsteed told Lowthorp that he,

'looked upon his imparting what he had deduced from them to Dr Gregory and the Captain [Halley] as a greater breach of promise than if he had imparted the observations themselves, and so would not request that as a favour which was my due'.

He was resolved to give Newton no more lunar observations until he should

'withdraw what he has imparted to others, or stop their reflecting discourses, and own before Sir Christopher [Wren] what he has already received, and what I then imparted to him'¹⁶⁷.

¹⁶⁶ See, for example, Newton to Flamsteed: 17.11.1694 and 16.2.1695 NCIV 480 & 494 46-48, 86-88.

¹⁶⁷ Flamsteed to Lowthorp: 10.5.1700, Baily op cit(12) 174-76.

It seems unlikely that, even if the breach of faith were not mentioned between them, Newton could have remained unaware of Flamsteed's annoyance.

Flamsteed clearly expected that Newton would require further observations, saying that the theory of the moon owed more to them than to the law of gravitation, and he was sceptical of the claims 'given out at Oxford' that the lunar theory was then complete and independent of Flamsteed's observations¹⁶⁸. A few months later, in October, 1700, Flamsteed wrote 'The state of the Observatory' a brief account of the history of the observatory and the observations made there. Here he mentions that his lunar observations were furnished to 'persons of known ability and skill' from whom he hopes more accurate lunar tables will be forthcoming. In a footnote to this passage he names Newton, and explains that he did not do so in the paper

'because some people, to ingratiate with him, have been very loud about what he has done in the theory of the moon; and thereby caused others to dun him about it: whereas it is not yet complete, and will perhaps require observations continued for 20 years, to be accounted from the year 1698, before it can be'¹⁶⁹.

In other words, Flamsteed's wrath had been calmed, and he believed that the results given to Halley and Gregory were in a very incomplete

¹⁶⁸ Ibid 176.

¹⁶⁹ Ibid 187-89, 191.

form, on which Newton wished to do a lot of further work before making them public. In the letter to Lowthorp, Flamsteed had mentioned his expectation of seeing Newton over the summer and it seems probable that Newton had given him this impression¹⁷⁰.

Flamsteed did not see the Astronomiae before publication, although Gregory had resolved to discover Flamsteed's opinion of it in the summer of 1701¹⁷¹. When he visited London on the next month Gregory discussed the moon's motions with Newton¹⁷², who cannot have been unaware of Gregory's intention to publish the theory of the moon, nor of the annoyance this would cause Flamsteed. For, as we have seen, this theory was presented by Gregory, and later by Whiston, as a virtually complete theory, and not as the first draft which Flamsteed now believed it to be. If Flamsteed had been shown the Astronomiae he would have taken exception not only to the arguments against his measurements of the parallax of the pole star, but also to this further breach of faith. Gregory's decision not to show it to him, made apparently while visiting Newton and Halley, suggests that these two may have been more aware than Gregory of what Flamsteed's reaction would be.

The publication of the lunar theory did indeed reawaken Flamsteed's anger, for on 10th November, 1702, Gregory reported that he had challenged Newton on the matter, producing letters which

¹⁷⁰ Ibid 176.

¹⁷¹ Flamsteed to Thornton: 18.2.1703 ibid 748; 21.5.1701 A68² partly NCIV 634 354-55.

¹⁷² 3.6.1701 RG fo 79.

promised the theory to him¹⁷³. Yet again, Gregory had become involved in the arguments between Newton and Flamsteed.

Whether Gregory and Halley

'persuaded Newton to break his promise and to permit Gregory to use the results [of Newton's work on the moon] in his Astronomy'

as More concludes¹⁷⁴, or whether Gregory, after Newton had freely given him the theory for his Astronomiae, was himself persuaded by Newton and Halley to keep his intentions of publication from Flamsteed, cannot now be answered definitely. However, Gregory's intention to discover Flamsteed's opinion of his Astronomiae implies that, whatever Halley's part may have been, Gregory was still unaware in May, 1701 that there had been any promise to Flamsteed to be broken.

¹⁷³ E106 Hiscock 13.

¹⁷⁴ Louis Trenchard More Isaac Newton (New York, 1934) 435.

5.6 Book five: Comets

In the fifth book, Gregory deals with the physical existence and orbital paths of comets. He begins by examining the opinions of all ages; Aristotle, the Pythagoreans, Plutarch, Seneca, Democritus and Apollonius are credited with at least suspecting the periodicity of comets, but this was denied by the Peripatetics, who had no room for comets among the spheres and orbs of their incorruptible universe. The observations of Brahe and Kepler restored the comets to their supralunary position, and this was further confirmed by Cassini's observations of the comet of 1680.

The orbit of comets is not yet a decided point. Kepler, Descartes and Hevelius assign them rectilinear paths, but Cassini suggests that their orbits are periodic, concave towards the earth. He seems, though, to regard this merely as a calculatory device,

'being too cautious a Natural Philosopher to affirm or define anything concerning that matter'¹⁷⁵.

In 1682, James Bernoulli published a *System of Comets*, suggesting that they were the satellites of an invisible planet whose orbit lay about the sun¹⁷⁶. However, Gregory inclines to the view that comets, as laid down in proposition 35, book 1, follow conic sections with the sun in one focus, which is both agreeable to the general principle of simplicity in nature, and capable of taking on all the appearances of the other theories proposed. This is in sharp

¹⁷⁵ Ast(26) 5 1 proposition 2 700.

¹⁷⁶ James Bernoulli Conamen novi systematis cometarum (Amsterdam, 1682).

contrast to Gregory's conclusions in his astronomy lectures of 1685 when he had decided that Descartes' hypothesis of non-returning planets drifting from one vortex to the next was more probable and more in agreement with the phenomena than Cassini's hypothesis of periodical bodies in closed orbits. This change of mind can only be due to the influence of Newton's mathematical analysis of gravitational forces and cometary orbits, but Newton's name is not once mentioned in this context.

The third proposition examines the expansion of the air, an analysis which Keill was to criticise for its assumption (unacknowledged in Gregory's first draft) of uniform gravity¹⁷⁷. After proving here, as in the Oxford astronomy lectures¹⁷⁸, that a one inch diameter sphere of our air would, at a height of a semi-diameter of the earth, expand to fill the solar system, he considers the various explanations which have been proposed for cometary tails. He examines and rejects the opinion of Peter Apian, Cardan, Tycho and Snell that it is the effect of the sun's rays refracted through the comet's head, and the opinion of the Cartesians that it is due to a refraction effect between the comet's head and our eyes. Nor is James Bernoulli's suggestion that it is due to the satellites of his invisible planet gathering exhalations of the other bodies in the vortex, acceptable, Gregory believes that the comets themselves exude a tenuous vapour, but he objects to Hooke's idea of a continual absorption of cometary matter by the ether, whereupon it loses

¹⁷⁷ Misc. 7.

¹⁷⁸ Lecture for 22.10.1696.

its gravity and flees from the sun, forming a tail, on the grounds that no bodies we know on earth can so alter their amount of gravity, and there is no reason to suppose that other bodies do. This hypothesis of an exhalation of gas was supported by Aristotle, who believed it to be on fire. However Kepler, James Gregory, Hevelius and especially Isaac Newton have produced feasible hypotheses which do not assume that the gas is burning. The foregoing proposition on the expansion of air is now used to show that only a small amount of vapour need be given off to produce a considerable tail. From all this Gregory draws an interesting corollary¹⁷⁹; if the exhalations of a comet should mix with our atmosphere, they may well have a strong effect on the inhabitants and produce of the earth, and the happenings traditionally associated with comets might well be the result of their passing. Indeed, it would be unworthy of a philosopher to deny the possibility. However, he does not, as he had at one time planned¹⁸⁰, discuss Whiston's theory of the rôle of comets in causing the flood.

The remaining three sections are concerned with the determination of a comet's position. This is done first on the assumption that its path through the sphere of fixed stars is a great circle, and then, after giving Wren's lemma on drawing a line to cut four others given in position so that its three segments thus cut will bear a given ratio to each other, on the assumption that the path is rectilinear. He was later criticized (he does not say ^{whom} ~~who~~ by)

¹⁷⁹ Ast(26) 5 1 corollary 2 to proposition 4 716.

¹⁸⁰ A60.

for including a proposition on this false assumption, and for the insolubility of the accompanying geometry under certain conditions. However, he decided that this insolubility would not occur if the orbits really were rectilinear, and its existence merely underlined the falseness of the assumption. No change need therefore be made in a new edition¹⁸¹. Next Gregory assumes a parabolic or elliptic path, deducing and using many of the results on comets in book 3 of the Principia. He determines orbits both arithmetically and graphically and explains Mouton's method of differences. Finally he explains how the rule of false position may be used to correct the initial determinations. The last section of book 5 considers the helio- and geo-centric positions of a comet and the use of tables in establishing these, and it is here that he describes Halley's work on comets, and gives in particular his results on the comet of 1680 as set out in his letter to Newton. Halley and Newton had wished Gregory to omit something he had intended to say here¹⁸², but there is nothing to tell us if anything has been omitted or, if so, what it was.

¹⁸¹ E148.

¹⁸² 21.5.1701 A68² partly NCIV 634 354-55.

5.7 Book six: Comparative astronomy

The sixth book, on comparative astronomy, was the final book to be completed, being finished on 12th February, 1700¹⁸³ and it was also the last to be planned. Not until August, 1699, by which time Gregory had essentially completed books 1-4, does he note down his firm intention to gather the comments he had intended to scatter through the first five books into one book¹⁸⁴. Later he considered splitting it into two, or even omitting it altogether¹⁸⁵.

Gregory had used this device in his Edinburgh Astronomy lectures, when explaining the Ptolemaic divisions of the stars. An astronomer on Jupiter, he says, would posit a similar system, with Jupiter stationary in the centre, for his convenience in calculation. It might happen that over the years others would be deceived by the form of speech surrounding this assumption and take it for the truth, acting as they do on Earth who proclaim the physical truth of the Ptolemaic system. By thus considering the analogy with Jupiter, we may better understand the hypothetical nature of the Ptolemaic sphere, and Gregory's original intention was to introduce the concept here in a similarly limited rôle¹⁸⁶. However, as the writing progressed, his treatment was extended to include the view from the other planets, the sun, moon and comets. Yet it is for the same basic end - a clearer understanding of the relationships

183 Gregory's manuscript of book 6 is at the end of folio B, EUL.

184 Misc. 70.

185 A56², misc. 7.

186 A60.

between the celestial bodies and between their true and apparent motions - that he now intends to speak of 'such empty appearances, and never seen, or likely to be seen' as the apparent universe from positions outside our earth¹⁸⁷. Plutarch's Face in the Orb of the Moon, Kepler's Somnium and Huygen's Theory of the World have used similar devices, but, unlike the latter two authors Gregory does not wish to imply a belief in inhabitants of these other worlds, but only to use the supposition as a means of making astronomy clearer. Some of Kepler's conclusions, too, he has been forced to change: they were false because Kepler did not properly understand the moon's librations.

Gregory considers the sun, planets and comets and satellites explaining the systems an observer on each would establish, and the methods available to him for discovering the true system or calculating the physical constants of the solar system in terms of particular distances. He considers lunar astronomy very fully, giving many of Kepler's conclusions. Finally he compares the relative ease with which an observer might, from these positions establish the true system. This is easiest from the sun, and somewhat easier from a satellite than a planet. Although the observer on the satellite has initially a more intricate system to penetrate, he will have the advantage for unlike the planets (says Gregory) the satellites have no atmosphere to hinder observation, and their primary planets provide universal clocks. However, Gregory comfortably concludes that of all the primary planets, none is so

¹⁸⁷ Ast(26) 6 preface 811.

well fitted for determining the true nature of the solar system, as our Earth.

5.8 The reception of the Astronomiae

One of Gregory's earliest notes for this book was

'To consult Mr Newton about the design, method and particular difficultys'¹⁸⁸.

As we saw in 5.1 he did just this throughout the period when the book was written. Not only did the preface and the lunar theory derive directly from Newton, but so did the discussion in book 1, section XI of the forces necessary to maintain the Copernican system and its rivals. Among other topics, Gregory also discussed with Newton the determination of planetary orbits, the inclination of the moon's orbit, the diameter of the stars and the problems of refraction and parallax¹⁸⁹. The work not only had Newton's approval, but his active co-operation (as well as that of other Newtonian scientists).

Yet neither Gregory nor Newton ever made public the part the latter had played in creating the Astronomiae. Gregory's motives for wishing it to appear his sole creation are clear; Newton was probably reluctant to take public responsibility for a work which he had not checked in every detail. (The episode of Cassini's orbit (5.4.2) is strong evidence that Newton had not studied all the work.) This situation allowed Hearne to remark in 1705 that

'Men well skill'd in Mathematics scruple not to say that

Dr Gregory has stole most of his Astronomy from Isaac Newton,

¹⁸⁸ Misc. 70.

¹⁸⁹ Misc. 70, RG fo 79, C62, RG fo 62.

whom he has mentioned wth some little acknowledgm^t but not so often as he should have done: w^{ch} as 'tis said has put S^r Isaac on a new Edition of his Principia etc¹⁹⁰.

Without the evidence of Gregory's manuscripts, there was no reason (other than Hearne's liability to be carried away by personal spite) to dispute these charges. In 1934, L.T. More quoted Hearne, and remarked that

'Newton was intolerant of any encroachment on his preserves, and he may have been piqued because Gregory did not acknowledge more explicitly his indebtedness'¹⁹¹.

After studying the memoranda in Gregory's workbook E, Hiscock argued against this view. He urged Gregory's strong 'altruistic interest' in the preparation of the new edition of the Principia and the continuing warm friendship between the two men as evidence that Gregory's book did not anger Newton¹⁹². We can now put the case even more strongly: why should Newton be angry at the appearance of a work when he had been consulted at every step in its preparation?

One who was angry at the work's appearance was John Flamsteed. He complained to Thornton

¹⁹⁰ Hearne op cit(114) 90.

¹⁹¹ More op cit(174) 532.

¹⁹² Hiscock VIII.

'Mr Halley saw his [Gregory's] book before it was printed:

I was not vouchsafed the sight of it; the reason is plain
to you'¹⁹³.

His anger at Gregory's discussion of the parallax of the pole star and his publication of Newton's lunar theory were discussed in 5.4.3 and 5.1.1. The first was the main complaint in his bitter letter to Caswell, which Sharp, Thornton, Wallis and Gregory also saw¹⁹⁴. Another letter to Caswell and one to Thornton also criticised the work¹⁹⁵. By this time Flamsteed's and Gregory's personal animosity towards each other blinded the judgement of both men. Yet the contrast between Flamsteed, the practical astronomer, and Gregory, the theoretician, also was an important part of their mutual criticism. It was in this context that Flamsteed called Gregory a 'closet astronomer'¹⁹⁶.

In other quarters the work was well received. The anonymous review in the Acta, although it suggested that the calculatory rules might be extended, gave a full and complimentary account of 'this most excellent work'¹⁹⁷. Keill's review in the Transactions went further:

¹⁹³ Flamsteed to Thornton: 18.2.1703 Baily op cit(12) 747-48.

¹⁹⁴ See n 139.

¹⁹⁵ Flamsteed to Caswell: 30.7.1702 Baily op cit(12) 203-05,
Flamsteed to Thornton: 18.2.1703 ibid 747-48.

¹⁹⁶ Ibid 204.

¹⁹⁷ AE (October, 1703) 452-62.

'What the World has hitherto wanted, the learned Dr Gregory has supplied it with a compleat System of true and Physical Astronomy: and as the last Ages have been sufficiently furnished by the Ancients with the Elements of Geometry, so without question the future will have recourse to this Book for those of Physical and Geometrical Astronomy'¹⁹⁸.

It was Keill, too, who said the work would endure as long as the sun and the moon¹⁹⁹. Whiston's astronomy lectures were at once influenced by the appearance of the work, and used it on several occasions. In particular, his treatment of Kepler's second law and the devices used to approximate it seems to derive from Gregory's and he quotes Newton's theory of the moon from the Astronomiae²⁰⁰.

Not only the lecturers, but the students, too, read Gregory's text. Robert Green, a tutor of Clare College, Cambridge, recommended it for pupils in their fourth year. He was, though, a non-Newtonian, and while recommending Newton, Gregory and Whiston, gave his chief commendation to Bouillieu²⁰¹. Daniel Waterland's Advice to a young student was written some years before its first publication in 1729, and it also suggests Gregory's Astronomiae as a work for final year students²⁰².

¹⁹⁸ PT 23 (January, February, 1703) no 283, 1312-20, 1312.

¹⁹⁹ Keill op cit(1) preface.

²⁰⁰ Whiston op cit(162) lectures 26-28, 30, 31.

²⁰¹ W.W.R. Ball History of mathematics at Cambridge (Cambridge, 1889) 95-96.

²⁰² The works of the Reverend Daniel Waterland (Oxford, 1823-28) 6 301-324, 321.

It is worth emphasising, too, that this book introduced more than the doctrines of Newtonian science. Kepler's work especially was widely discussed, as were Leibniz' harmonic vortex and James Bernoulli's theory of comets. All opposing theories were rejected by Gregory in favour of Newtonian ones, but the discussions of them introduced them to a wider audience. Cassini, Hevelius and Huygens were only some of the other non-Newtonians, whose work, especially in observation, was reported in the Astronomiae.

Gregory's use of Ward's and Bouilleau's devices was also to prove significant. As Thoren persuasively argues, these and similar devices had been used in England throughout the latter half of the seventeenth century, not because Kepler's law of areas was unknown, but because it was so difficult to use²⁰³. Mercator, in an article of 1669, with which Gregory was familiar when writing his Astronomiae, had presented equant devices as a convenience in calculation, approximating to the accurate law of areas²⁰⁴. Gregory continued this tradition in his Astronomiae and

'seems to have been the one who set the pattern for the discussion of equant hypotheses as explicit numerical approximations to Kepler's second law in the Newtonian tradition'²⁰⁵.

However, the major criticism of Gregory's work lay in its

²⁰³ Victor E. Thoren 'Kepler's second law in England' British Journal for the history of science VII pt.3 (November, 1974) no 27.

²⁰⁴ PT V (March, 1670) no 57 1168-75.

²⁰⁵ Thoren op cit(203) 256 n 58.

difficulty. Keill's Astronomy of 1718 praised Gregory and his work, but pointed out that his Astronomiae was too difficult for students, who may be able to learn astronomy although ignorant of the geometry assumed in the text. Also, he found Gregory's arrangement, where the motions are considered along with their causes, a confusing one. He preferred to describe all the motions and the appearances they gave rise to first, before studying physical causes²⁰⁶. Waterland recommends Keill's Astronomy and Physics for second year students, calling the former 'plain and intelligible'²⁰⁷. He considers Gregory's Astronomiae and Newton's Optics

'more difficult to understand than any before mentioned, requiring much thought and close application to be a master of them'²⁰⁸.

Flamsteed, unsurprisingly, voiced these opinions most strongly when he wrote to Thornton,

'You thought to have found Mr Newton's Principles made easier by [Gregory's Astronomiae]: but except you read Mr Newton's preliminaries, you would not understand Dr Gregory'²⁰⁹.

²⁰⁶ John Keill Introductio ad veram astronomiam (Oxon, 1718); the English edition of 1778 suggests that these causes may be found out from Gregory's Astronomiae.

²⁰⁷ Waterland op cit(202) 318.

²⁰⁸ Ibid 321.

²⁰⁹ Flamsteed to Thornton: 18.2.1703 Baily op cit(12) 747-48.

This is, of course, exaggerated, but the book was certainly not easy.

Gregory had intended his work to be useful to learned or unlearned, but he was aware that it would assume a knowledge of geometry and had planned it primarily as a book from which to lecture²¹⁰. It is unsurprising, therefore, that Keill and Waterland found it too difficult for young students. As a popular exposition of Newtonian astronomy it was eclipsed in the eighteenth century by more elementary texts, particularly Keill's Astronomy²¹¹.

Yet in 1795, Hutton could still refer to it as the work which, along with Newton's Principia, carried theoretical astronomy 'to the highest perfection'²¹². While superseded as a text for undergraduates by works such as Keill's and Whiston's, the work was well-respected in the eighteenth century. On Gregory's death it was even more than this. It was the exposition of Newtonian astronomy, the work which would outlast sun and moon, and through which Gregory's reputation would do the same.

²¹⁰ A60.

²¹¹ Keill op cit(206).

²¹² Charles Hutton. A mathematical and philosophical dictionary (London, 1796) 2 vols 1 160.

Appendix to Chapter 5; taken from A60 & E87-88.

Source books for Gregory's Astronomiae

Kepleri

Epitome Astr: Cop: (1)

Comm: in Stellam Martis (2)

Harmonice Mundi - Ptolemaei (3)

Mysterium Cosmographium (4)

Tabb: Rudolphinae (5)

Ast: pars Optica (6)

Somnium Astronomicum (7)

* De Cometis (8)

Nic: Mercatoris Inst: Astron: (9)

Jac: Greg: Probl: in Opt: Prom: (10)

Is. Newtoni Princip: Philos: (11)

Bullialdi Astr: Philolaica (12)

Street's Tabb: Carolinae (13)

De La Hire Tabb: [solis] et [lunae] (14)

Lansbergii Tabulae (15)

Copernicus de Revolutionibus (16)

Ptolemaei Almagestum (17)

*Jac. Bernoulli Conamen Comet: (18)

Hallei Astron. quaevis in transact: (19)

†Circa fixarum observationes Hevelio fidendum et Flamstedius consulendus
(20)

Cassini Astronomia quaevis et de la Comete, praecipue de Satellitibus
[Jovis] et [Saturni] (21)

*Galilei Nuncius Sydereus (22)

Horroccii et Crabtrei posthuma (23)

- Hugenii Systema Saturnium (24)
- Moutoni Astronomica (25)
- Steveni Astronomia (26)
- De Chales Astronomia etc. (27)
- Gassendi Astronomicae (28)
- Wardi Astronomia Geometria (29)
- *Hevelii Omnia Astronomica (30)
- *Bernoulli de Cometis (31)
- *Mr Hugen de la lumière etc. (32)
- *Longomontani Astronomia Danica (33)
- *Horocii Tabb. [solis] et [lunae] in Sir Jonas Mores works (34)
- *Fatio de la nouvelle lumière (35)
- *Sherburn's Manilius preface (36)
- *Hooke's Lectures particularly his Cometa (37)

*Books added when list was copied into workbook E.

+This remark was omitted from the list in workbook E.

Index to Appendix to Chapter 5: Sources for the Astronomiae

- (1) Johannes Kepler Epitome Astronomiae Copernicanae... 3pt.
(Lentiis ad Danubium Francofurti, 1618-22).
- (2) Johannes Kepler Astronomia Nova, seu Physica Coelestis,
tradita Commentariis de Motibus stellae Martis ex Observa-
tionibus ... Tycho Brahe ... (Prague, 1609).
- (3) Johannes Kepler Harmonices Mundi Libri V Appendix habet
comparationem hujus Operis cum Harmonices Cl. Ptolemici libro
III... (Lincii Austriae, 1619).
- (4) Johannes Kepler Prodromus dissertationum mathematicorum,
continens Mysterium cosmographium... (Tubingae, 1596).
- (5) Tycho Brahe Tabulae Rudolphinae ... Tabulas ipsas ... morte
authoris sui anno MDCL desertas ... continuavit ... perfecit
absolvit: adeoque causarum & calculi perennis formulam
traduxit Ioannes Keplerus etc. 2pt. (Ulm, 1627).
- (6) Johannes Kepler Ad Vitellionem paralipomena, quibus Astronomiae
pars optica traditur ... (Francofurti, 1604).
- (7) Johannes Kepler Somnium, seu opus posthumum de astronomia
lunari ... (Francofurti, 1634).
- (8) Johannes Kepler De Cometis Libellis III ... (Augustae
Vindeliiorum, 1619).
- (9) Nicolas Mercator Institutionum Astronomicarum ... (London, 1676).
- (10) James Gregory Optica Promota ... cui subnectitur Appendix,
subtilissimorum Astronomiae Problemata resolutionem exhibens
(Londini, 1663).
- (11) Isaac Newton Philosophiae Naturalis Principia Mathematica
(Londini, 1687).
- (12) Ismael Bouilleau Astronomia Philolaica (Parisiis, 1645).
- (13) Thomas Streete Astronomia Carolina. A new theorie of the
celestial motions (London, 1661).
- (14) Phillipe de la Hire Tabularum Astronomicarum ... (Paris, 1687).
- (15) Philip Landsberg Tabulae motuum coelestium perpetuae ...
(Middelburgi, 1632).
- (16) Nicholas Copernicus ... De Revolutionibus Orbium Coelestium,
libri IV ... (Noninbergae, 1543).
- (17) Gregory might have used any one of several Latin editions of
Ptolemy's Almagest.

- (18) Jakob Bernoulli Conamen novi systematis cometarum ...
(Amsterdam, 1682).
- (19) Halley's papers in the Philosophical Transactions included many of astronomical observations and on the variations of a magnetic needle, and, in particular "A Direct and Geometrical Method by which the Aphelia, Excentricities and Proportion of the Orb of the Primary Planets are found, without supposing the Equality of the Angle of Motion at the other Focus of the Planet's Ellipse" Philosophical Transactions, 1676, XI p.683. Gregory had a copy of this last article among his papers.
- (20) 'Hevelius is to be trusted for observations of the fixed stars, and Flamsteed is to be consulted.' For Hevelius' works, see (30).
- (21) Cassini published verymany astronomical papers, most of which are contained in Recueil d'Observations ... avec divers Traitez Astronomiques par Messieurs de l'Academie Royale des Sciences 2 vols. (Paris, 1693) or in Mémoires de l'Académie Royale des Sciences depuis 1666 jusqu'en 1699 (Paris, 1730) vol. IX 'Oeuvres Diverses'. Gregory's access to the Mémoires was erratic, but he seems to have had access to the Recueil while compiling his astronomy. (Misc. 70., has his notes on Cassini's Tract 'De l'origine and du progrès de l'Astronomie ...'). Cassini had spent many years observing Jupiter and Saturn with their satellites, and had posited a 'cometical zodiac', or narrow band of the heavens within which comets travelled.
- (22) Galileo Galilei Sidereus Nuncius (Venice, 1610).
- (23) Crabtree's posthumous works were partially included in Jeremiah Horrox Opera Posthuma ed. John Wallis (London, 1672, 1673, 1678).
- (24) Christian Huygens Systema Saturnium, sive de causis mirandorum Saturni phenomenon et comite ejus planeta novo. (The Hague, 1659).
- (25) Gabriel Mouton: Observationes diametrorum solis et lunae apparentium... Dissertatio de diueris naturalium inequalitate ...
(Lyons, 1670).
- (26) Simon Stevin Hypomnemata mathematica ... a Simone Stevino conscripta et e Belgico in Latinum a Wil. Sn. [Willebrod Snell] conversa 5 vols. (Leiden, 1605-08).
Or possibly Les Oeuvres mathematiques de Simon Stevin deBruges ... corrigé et augmenté par Albert Girard ... (Leiden, Elzerier, 1634), which Sarton considers was the most used edition of Stevin's work. [George Sarton 'Simon Stevin of Bruges' Isis 1934, 21 241-303; p.268.] An earlier French translation by Tunning (1605-08) had not contained the final 3 books of volume 1, which constitute the work's contribution to astronomy.
- (27) Dechaies Cursus seu Mundus Mathematicus 3 vols. (Lyons, 1674) or ed. by Amati Varin in 4 vols. (Lyons, 1690)

- (28) Pierre Gassendi Institutio Astronomica juxta hypothesis tam veterum quam Copernici et Tythonici (Paris, 1647).
- (29) Seth Ward Astronomia Geometrica ... (London, 1656).
- (30) Hevelius published prolifically, mainly accounts of his observations. His most widely read work was a catalogue of the fixed stars (see (20)), published by his wife after his death; Prodromus Astronomiae ... (Gedani, 1690). The other more well-known ones included Selenographia, sive Lunae Descriptio ... (Gedani, 1647), Cometographia, totam naturam Cometarum ... exhibens (Gedani, 1668), Machinae Coelestis. Pars prior ... et ... pars posterior (Gedani, 1679), Annus Climactericus ... (Gedani, 1685).
- (31) The only work on comets by either Bernoulli which Gregory would have had access to in 1697 appears to have been Jacques Bernoulli's Conamen novi systematis cometarum ... (Amsterdam, 1682) already noted down by Gregory in (18) above.
- (32) Christian Huygens Traité de la Lumière ... avec un discours de la cause de la Pesanteur (Leiden, 1690).
- (33) Christianus Longomontanus Astronomia Danica ... 3 pts. (Amstelodami, 1622).
- (34) Sir Jonas Moore A new systeme of the Mathematicks (ed. by W. Hanway and J. Potenger) 2 vols. (London, 1681) contained John Flamsteed 'The Doctrine of the Sphere', wherein were Flamsteed's editions of Horrocks' tables. Flamsteed also edited the lunar tables in Horrocks' Opera Posthuma see (23).
- (35) N. Fatio de Duillier Lettre à M. Cassini ... touchant une lumière extraordinaire qui paroît dans le ciel depuis quelques années (Amsterdam, 1686).
- (36) Sir Edward Sherburne The Sphere of M. Manilius made an English poem: with annotations and an astronomical appendix by E. Sherburne (London, 1675).
- (37) Robert Hooke Lectiones Cutlerianae (London, 1679).

Gregory's work, classified by type

<u>Expositions</u>		<u>Original Work</u>	<u>Observations</u>
3.1 - 3.7	<u>Exercitatio</u>		
2.5 - 2.11	Edinburgh lectures * (including <u>Opticae and Geometry</u>)	3.8 - 3.14 'Second method'	Observations of an
2.13	Oxford lectures *	4.2 Florentine problem	eclipse of the sun,
1.5	<u>Notae</u> *	4.5 Brachistochrone *	in PT 21 (Sept. 1699)
4.3	Tract on fluxions *	4.5 Descent in a cycloid	no. 256 330-1.
4.4	Workbook E *	4.6 Catenary (and catenary answer)	
Chap. 5	<u>Astronomiae</u>	5.3.1 Model of atmospheric refraction *	
1.10	<u>Euclid</u>	4.2 Hippocrates' lunula	
4.7	planned work 'Contactus et Telragonismus' *	5.4.2 Cassinian orbit (in <u>Transactions and Astronomiae</u>)	

* indicates an unpublished work, paper or body of research.

Of Gregory's published work I have omitted only his papers in the Transactions defending his uncle, James Gregorie, against Abbe Galloys' charges of plagiarism, his review of Viviani's De Locis Solidis, also in the Transactions and his life of Wallis, which may have appeared in an historical dictionary. Full titles of published works are given in Appendix 1, Chapter 1. The numbers beside each entry indicate the place in this thesis where it is discussed.

Chapter 6

Concluding remarks

In this thesis I have divided Gregory's work according to the period in which it was written. To conclude, however, I have adopted an alternative grouping into three categories; expositions of the work of others, original work and observations (see table opposite). Like any such grouping this must be to some extent artificial. For example, I have included the lectures as expositions when they, and especially the optics lectures, contain some original work. Nor are all the propositions of workbook E derivative. Alternatively, we could see Gregory's work on the catenary as an exposition of the work of the continental scientists, or his 'second method' as an exposition of the work of Isaac Newton. However, a similar case could be made out against all Gregory's original work, and I have regarded as original those which he so regarded and which contained a high proportion of his own mathematics, even although, in almost every case, the answer was known to him in advance.

Below I have discussed Gregory's undoubted abilities as an expositor, the flaws in his original mathematics and the paucity of his observational measurements. These can all be partly linked with his search for unity and order in the natural world, and I have looked first at this aspect of Gregory's thought. Finally, I have examined the views of his ability held by his contemporaries and immediate successors, and made some remarks on the form and scope of his influence on future scientific developments.

6.1 Order and harmony

Gregory found in mathematics an order and harmony which seemed

to reflect that of the physical world. As early as 1683, in the inaugural speech he gave at Edinburgh, he had remarked on these qualities¹. The applicability of mathematics, which he also emphasized in 1683, evidenced this order in God's world. When answering charges of atheism before the Visitation Committee of 1690, he had instanced the symmetry and harmony which he, as a mathematician, saw in the physical world, as a proof of God's existence². That these were not merely platitudes, but were deeply held beliefs is shown in the influence they had on his scientific interests.

In March, 1695, he made a note

'It would be worthwhile to write about physics; where maximina and minima are used by nature, where harmony etc. or where the plan of the creator shows most clearly'³.

He hoped to find material for such a work in Newton's Principia and Kepler's writings, but unfortunately he never put this plan into action.

It may have been significant that this note was made just before the publication of his Optics, for it was in the behaviour of light that Gregory saw these principles working most clearly. He rarely mentioned Newton's theory of the decomposition of white light, but was concerned rather with the axiom that nature always acts in the easiest fashion. As I discussed in 2.6.4, he generally took this axiom as the least time principle; that is, nature acts in such a way

¹ SUL MS QA33 G8D1.

² B25.

³ 6.3.1695 Oxford RG fo.163.

as to complete an act or a motion in the least possible time. The ancients had shown that this principle led to the correct law of reflection, and Fermat had shown that it also did so in the case of refraction. Gregory used Newton's version of Fermat's analysis in both his Optics and his Tract on Fluxions. His attempt to find the brachistochrone and his analysis of atmospheric refraction both indicate a wish to extend this principle.

Newton's law of universal gravitation had linked such diverse phenomena as Kepler's three laws of planetary motion, the behaviour of liquids in a vortex and the speed of sound. His theory of fluxions and fluents contained the fundamental theorem of the calculus which links the inverse operations of integration and differentiation. Gregory saw the least time principle as another such unifying law to be used in the analysis of nature. He seems to have made no attempt to argue its general applicability, but either accepted it unquestioningly as another basic law of nature or at least hoped it would emerge as such when enough examples of its operation had been examined.

However, Gregory had seen harmony and order in the physical world before he read the Principia; in his optics lectures of 1683 he described Fermat's use of the principle of least time in analysing refraction as 'beyond all doubt, reducing this theorem to the test of geometrical analysis'⁴. Newton's Principia may have given an additional impetus to this aspect of Gregory's thought or it may have influenced the direction which it took, but it did not instigate it. Rather, Gregory's search for harmony in the physical world led to his enthusiastic response to Newton's work.

⁴ 'Lectiones Opticae' scholium to prop.14.

This vision of a universal harmony and the belief that it reflected God's will was the common heritage of Gregory and Newton. The mathematical interpretation which Gregory gave it was influenced by his study of authors such as John Wallis, but also by his natural inclinations and abilities. Newton's outstanding success in mathematically analysing the physical world simply confirmed Gregory in his ideas.

We may argue, too, that the science of astronomy became attractive to Gregory only after Newton had shown the success of his laws in establishing its rules. Gregory's Edinburgh lectures on astronomy were almost wholly based on Mercator's Institutiones; his Oxford lectures drew on a range of sources and indicated a much wider reading. At Edinburgh his research was all directed towards the purely mathematical study of integration by series; only among his Oxford papers do we find any studies of astronomical topics for their own sake.

Of course, influences other than a heightened appreciation of the subject were at work. The Oxford lectures were given to a more sophisticated audience with different expectations from those of the teenagers who attended the Edinburgh lectures. At Oxford, Gregory was professor of astronomy and not of mathematics in general; presumably more astronomical work was therefore expected of him. He was also meeting in England for the first time enthusiastic observational astronomers such as Halley and Flamsteed. The observational side did not appeal to him, but their enthusiasm may have kindled his own.

Nevertheless, we can see in Gregory's reaction to the Principia that of a mathematician who recognized in it the possibility of reducing the heavens to strict mathematical order. This might well

have given him a new satisfaction in astronomy which he had not found in empirically derived rules and certainly not in long, cold nights, spent observing the heavens.

In all Gregory's scientific work we find the approach of the mathematician, keenly aware of the necessity for system and order in mathematics and searching for similar qualities in the physical world. Thus, in all his writings on education, especially in mathematics, he emphasized the need for system and order, as well as for a solid mathematical foundation even in studies turned to purely practical ends. His Edinburgh lectures omitted work which was unnecessary in achieving these ends, such as Newton's which was too difficult for his students, but he did not attempt to make his lectures easier by omitting any of the basic mathematics, and simply stating practical rules. In the same spirit his popularizations of Newton's work were strictly mathematical and did not introduce experimental evidence.

All in all Gregory's work reflects his belief in a systematic physical world, derivable mathematically from a few fundamental principles such as universal gravitation. As he made clear in his Oxford lectures, he regarded gravity as explicable only as a quality instilled in matter by God. Not only gravity, though, but the harmony, system and order of the entire universe was evidence of God's presence. To base his work on any other assumption but a universal harmony would have been to deny God. The success of the least time principle in deriving the behaviour of light suggested that an harmonious universe would employ this principle widely, and so it became an important element in Gregory's study of the physical world.

6.2 Expositions

Gregory is most often remembered today for his expositions; for the Exercitatio of 1684 which expounded the methods of his uncle, James Gregorie, and, more especially, for the supposedly Newtonian lectures he gave at Edinburgh and the account of Newtonian astronomy in his Astronomiae. His edition of Euclid was for a long time the standard text. He also discussed Newton's work in the Notae, the tract on fluxions and workbook E; 'Contactus et Tetragonismus' would also have done so, but never grew beyond the planning stage.

As was discussed in chapter two, Gregory's Edinburgh lectures were far from Newtonian, but they were admirable expositions of standard contemporary works. Using authors such as Dechales, Wallis and Mercator, with the Cartesian influence especially marked in the astronomy lectures, Gregory introduced his Edinburgh students to a wide range of mathematical topics. He presented these so as to give a sound mathematical foundation to his practical goals. His success here was attested by the popularity of his lectures with generations of Scottish students.

The Notae, the Fluxions and the Astronomiae each illuminated a different area of Newton's work, and clarified much that had been left obscure. They can be faulted for their errors and omissions, but must nevertheless have helped many to understand Newton's work; the Astronomiae especially was well-read and well regarded throughout the eighteenth century, although it was never seen as an easy book.

In the early eighteenth century, John Keill ran experimental courses at Oxford on the Newtonian philosophy. His True Astronomy (first published, Oxford 1718) was based on these courses. Thus

Keill's teaching and Gregory's unpublished tracts provided complementary approaches to Newtonianism for Oxford students. Their texts on astronomy provided similar alternatives for the reading public. Gregory's works clarified, but never omitted the mathematical details; Keill's works found other arguments to offer the less mathematically sophisticated reader.

This contrast pinpoints Gregory's rôle as a popularizer of Newton's work. He did not consider the more metaphysical questions with which Keill's work was in a large part concerned; the definitions of matter or of absolute and relative space. Nor did he, like Whiston, Keill and others, consider the theological implications of Newtonian philosophy in any detail. He made no attempt to establish experimental proofs; the only experiment mentioned in connection with David Gregory was a demonstration to his Edinburgh students of the properties of a vacuum⁵. Only in his Oxford lectures did he discuss the possible causes of gravitational attraction; and then it was partly mathematical arguments which persuaded him of the impossibility of establishing a mechanical model.

Gregory was a mathematician, and to him it was mathematical analysis alone on which Newtonian philosophy stood or fell. To popularize Newton's science meant, to Gregory, to clarify his mathematics and bring them within the scope of the average student. Only once the mathematics was firmly established would he discuss its applications in solving the mechanical problems proposed on the continent or in establishing Kepler's laws. This, to Gregory, was Newton's philosophy and

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⁵ B24.

was, moreover, the only way in which any scientific principle could be established.

These expositions of Newtonianism were not limited strictly to Newton's work. The Astronomiae and, more especially, Fluxions and workbook E were based firmly on Newtonian principles, but also introduced much of the work then being done on the continent. The projected 'Contactus et Tetragonismus' would also have done so. The climax of the historical presentation of different methods in this work would be Newton's method of fluxions, but many other authors would be discussed. Although he intended it to include some of his own examples, Gregory's explicit plan was not to write an original work, but to clarify the obscurities in mathematical authors. Unfortunately, this work was probably too ambitious for him; without Newton's help he would have been unlikely to have handled his discussion of the contemporary continental work without error.

Otherwise, though, the work would have shown his strengths and abilities to their best advantages. Granted sufficient mathematical competence (which he generally possessed) Gregory had the application to study his subject until he understood it thoroughly, and was conversant with all the most suitable authors. With a clear view of his aims in writing, and of his readers' abilities, he could then (perhaps because the understanding did not come very easily to him) break down any areas of difficulty into their constituent steps and so clarify them. Moreover, he had the modesty to content himself with the relatively humble rôle of expositor. These qualities enabled him to write both the elementary and immensely popular Geometry and the difficult analysis of Newtonian science of his Astronomiae.

6.3 Original work

Unfortunately, the mathematical competence which was generally sufficient for Gregory to understand and explain the work of others, was frequently insufficient when he attempted more original work. He could, as with his papers in the Transactions on the Florentine problem, Hippocrates' lunula, descent in a cycloid or the Cassinian orbit, produce competent, even elegant, solutions of geometrical problems. He was undoubtedly helped in developing his 'second method' by what he knew of Newton's work, but his derivation was quite satisfactory, granted the general haphazard attitude of the time towards the convergence of series. Given the initial mathematization of the mechanical conditions in the catenary, his work on this, too, was quite adequate. None of these were trivial problems, but in all but two the answers were previously known. These two, descent in a cycloid and the Cassinian orbit, arose out of work in which he had previously erred, and were certainly not the most difficult of the problems. None of these studies were on a level of difficulty comparable with the mathematical work being done by Newton, Leibniz, de l'Hôpital and the Bernoullis, but they displayed competence in the new and difficult techniques which these others were developing.

In other parts of his work, though, we find that Gregory was far too ready to accept an inadequate proof which he believed led to the answer he required. These cases correspond to two situations; either he was supplying his own proofs to the work done previously by someone else, or else he had other, non-mathematical, reasons for believing a result was true.

The series in the Exercitatio for $\tan^{-1} x$ and the mathematization of the catenary are two examples of the first situation. The first

result had been stated by James Gregorie, but David's attempt to justify it revealed his lack of understanding (see 3.5.4). He knew the basic differential equation for the catenary from the work done on it by the continental geometers. Thus he could balance two erroneous physical assumptions in such a way that they combined to give this correct equation. The reduction of a physical situation to mathematical terms was one of Gregory's weak points and he avoided doing so whenever possible, preferring to take on trust the analyses of others.

The second situation is more interesting; here we have his work on Cassini's orbit in the Astronomiae, his search for the brachistochrone and his analysis of the curve of atmospheric refraction. All these examples reflect the influence on Gregory of his search for unifying principles in nature, which I discussed in section 1.

The Cassinian orbit promised to unify the physical exactitude of Kepler's area law with the mathematical exactitude of the empty focus equant device. Even when he discarded Cassini's erroneous argument in support of this promise, Gregory still found it attractive enough to search for an alternative proof. He was then satisfied with an argument of his own which involved quite unjustifiable assumptions about infinitesimal quantities; Gregory's belief in it was not based on sound mathematical reasoning, but on the attraction of the result it seemed to prove.

His discussion of the brachistochrone and of the curve of atmospheric refraction were influenced by his desire to extend the application of the least time principle, which was discussed in section one. The brachistochrone represented the path through which a particle descended under gravity in the least time. If this principle were to

have any universal application, we would expect this curve to be one of which nature makes use. (Just as a straight line can represent the swiftest path between two points, and nature uses this straight line for the path of a light beam). In identifying the curve with the catenary (the curve formed by a chain suspended at its ends), Gregory believed he was fulfilling this condition.

He first saw a tenuous connection between these two curves in his mathematical analysis and it may be that he would have accepted any natural curve which thus appeared to him. However, he gave several non-mathematical, almost metaphysical, reasons why the catenary was the required curve. The first was that, by its nature, the catenary, as in the case of refraction, related to transition in a minimum time. This statement is not exactly clear, but the reference to refraction leaves no doubt that Gregory was consciously searching for a connection with the least time principle. He then tried to justify this contention by suggesting that a heavy chain could be regarded as a weightless line along which a heavy point moves in a minimum time. In this attempt to analyse the brachistochrone we see explicitly Gregory's tendency to bolster up a weak mathematical argument by appeals to other considerations. It is hard to judge whether he was altogether sure of his proofs in this example, but he had certainly convinced himself.

It was in the analysis of refraction that Fermat had applied the least time principle so successfully, and it was natural for Gregory to hope that it would have a similar application to the curve of atmospheric refraction. By the time he began this analysis, Gregory knew that the brachistochrone was the cycloid, and it is not surprising that he deduced the same curve (although inverted) for the curve of

atmospheric refraction. To do so, he had taken a totally unrealistic physical situation and then made a false assumption in his mathematical analysis. Only the promise of harmony and order inherent in the answer he appeared to have achieved could have persuaded Gregory to accept those physical assumptions and that analysis.

6.4 Observations

In this third category we find only one set of published observations; of the eclipse of the sun on September 13th, 1699. In Gregory's papers there are several references to the observations and experiments of others (especially to Newton's experiments) but I have found only seven further references to his own observational efforts - and two of these might have been Halley's. None were extensive, and few were more than partially complete. We might add here, however, his records of unusual weather of which there are a few notes in workbook E, such as his description of the strong storm of November 26th, 1703 ⁶.

In workbook E we find details of Gregory's observations of an eclipse of the sun on May 6th, 1696, the most complete he left ⁷. Clouds had obscured the sky, but even when they dispersed, Gregory had been unable to perceive the moon during its immersion, although others observing the same phenomenon had done so. He explained that he was a little short-sighted! However, he remembered on this occasion that he had seen an eclipse in springtime at Edinburgh, when the moon had appeared red.

Perhaps it was this short sight again which hindered the observations of which Charlett wrote to Sloane on October 26th, 1697 ⁸. Gregory and Charlett had attempted to observe Mercury passing across the sun, but the sky had generally been too cloudy. When it did clear

⁶ E98 Hiscock 14.

⁷ E inside front cover. Also RG fo. 80.

⁸ Charlett to Sloane 26.10.1697 BM MS Sloane 4036 fo. 364.

for a minute, Gregory had been unable to perceive Mercury and suspected that its transit was then completed, although this was 15 minutes before the time given in the astronomical tables.

Next came his published account of the eclipse of 1699, of which he remarks 'I did not see the beginning of it', but gives no reason for this omission⁹. In 1703 a sunspot was seen in London, and Gregory watched its later appearances on June 16th and 18th¹⁰. He did not record any measurements of its size or path, but resolved to watch for it again, if it did not dissolve meantime. He made no further notes on it. An eclipse of the moon on December 12th, 1703 was obscured by clouds and Gregory gave only two approximate figures for the times of half and full eclipse¹¹. Two further eclipses in 1706, one on April 17th of the moon and one on May 1st of the sun were also obscured by clouds, and Gregory made only brief notes on them¹². By this time Edmond Halley was at Oxford, and these observations may have been his.

The Savilian statutes required the Astronomy Professor

'to take astronomical observations as well by night
as day ... and after reducing them all into writing ...
to leave them in the archives'¹³.

⁹ PT 21 (Sept. 1699) No. 256 330

¹⁰ E98.

¹¹ Ibid

¹² E168.

¹³ Christopher Ward Oxford University statutes (Oxford 1845) 274.

David Gregory's meagre list of observations (and those mostly obscured by cloud or hindered by myopia!) scarcely match up to this description. Newton's recommendation of him for the post had said

'He has been conversant in the best writers about
Astronomy and understands that science very well'¹⁴.

Newton spoke highly of his personal qualities and mathematical abilities, but said nothing of skills in observation. Apparently he regarded a theoretical background in astronomy sufficient for the Chair, and so, too, did the electors. So far as I can discover, the University raised no demur at Gregory's total neglect of the statutes on this point.

Perhaps it was partly to remedy this, however, that Edm⁹und Halley was appointed to the Savilian Chair of Geometry in 1704. With the support of Gregory and Charlett, he persuaded the University to build him the observatory which is still part of the old Savilian Geometry Professor's house in Oxford¹⁵. It seems that Halley took charge of observation at Oxford; in April 1706, he proposed alterations in the observing instruments to Gregory who acquiesced readily¹⁶.

Not everyone, though, was as complacent as the University over Gregory's lack of proficiency in observation. John Flamsteed had noticed it, and condemned him as a 'closet astronomer'¹⁷.

¹⁴ Newton to Charlett 31.7.1691 NCIII 366 154-5.

¹⁵ H.E. Bell 'The Savilian Professors' houses and Halley's observatory at Oxford' Notes and records of the Royal Society 16 (1961) 179-86.

¹⁶ El64.

¹⁷ Flamsteed to Caswell 30.7.1702 Francis Baily An account of the Reverend John Flamsteed (London, 1835) 203-5 p.204.

He regarded the criticisms Gregory had made of his work in the Astronomiae as the consequences of this outlook. In particular, Gregory had used Huygens' contention that the stars, even when magnified a hundredfold, have no sensible diameter, to argue that any parallax arising from the earth's annual motion will also be insensible. Flamsteed allowed that Huygens

'had tubes and glasses that did not lie always by him unemployed, as some instruments do that I got to be made for the Astronomy Professor at Oxford'¹⁸,

but nevertheless felt the Dutchman had been mistaken on this occasion. He pointed out that 'there are telescope glasses at Oxford, and conveniences for managing them' and suggested that Gregory avail himself of these to look at the planets and stars. Then he would perceive Huygens' mistake¹⁹. There is no sign that Gregory ever accepted this challenge.

There is some justice in Flamsteed's remarks; 'closet astronomer' was apt, and it was hard for the Astronomer Royal, to whom copious observations were the life-blood of astronomy, to see his work criticised by a myopic professor who but rarely held a telescope. Gregory's view was quite the opposite; observations might be important, but they were always secondary to theory. Just as his popularization of Newtonian philosophy emphasized its mathematical aspects and made little use of experiment; so his astronomy was that of a mathematician

¹⁸ Ibid 204.

¹⁹ Flamsteed to Caswell 5.9.1702 Ibid 205-8 p.207.

and not of a practical observer. As such, he sometimes failed to perceive the niceties of observational techniques or to assess the practical difficulties of a theoretically derived suggestion for observations.

In section one, it was suggested that Gregory's enthusiasm for astronomy came only when he saw Newtonian philosophy harmonize and unify the motions of the heavens. This attitude was totally opposed to that of Flamsteed, to whom an empirically derived theory was the only one likely to be of value. The dreaming mathematician and the practical observer typified two extreme approaches to astronomy and their professional conflict was inevitable, even had their personal relations remained friendly.

6.5 Contemporaries and Successors

Gregory was first and foremost a mathematician and secondly an expositor. His mathematics were highly competent rather than brilliantly original, yet they coloured his attitude to all other sciences. His optics, like almost all his work in the physical sciences, was restricted deliberately to aspects which could be mathematically handled, and even in medicine he inclined towards the iatromathematical school. His expositions were also coloured by this approach and were founded on solid mathematical bases.

Thus, the popularity of his expositions would reflect the general opinion of his mathematics and this appears to have varied. The popularity of his lecture courses in Scottish Universities shows that they were accepted for what they were; excellent expositions of sound authors. His texts on optics and astronomy also enjoyed a popularity which suggests that in the general view his mathematics was accepted as sufficient. However, it seems that Newton himself and some of his inner group of disciples had rather different views.

In 1691, Newton recommended Gregory to Oxford University as

'... in Mathematiques a great Artist ... He is not only acquainted with books but his invention in mathematical things is also good'²⁰.

Flamsteed's comments of 1703 were more grudging, but nevertheless paid tribute to Gregory's mathematical abilities. He wrote to Thornton,

'You tell me you have taken some propositions in trust,

~

²⁰ Newton to Charlett 31.7.1691 NCIII 366 154-5.

from the Doctor [that is, from Gregory's Astronomiae].

I believe you need not suspect his sincerity or abilities in anything of geometry, though his astronomy be poor'²¹.

By astronomy, of course, Flamsteed meant observational work.[~] By 'anything of geometry' in this context he meant the theoretical astronomy of the Astronomiae.

However, it appears unlikely that Newton would have endorsed Flamsteed's comments in 1703. Speaking of Gregory's hope in July, 1698, to publish Newton's 'Enumeratio linearum tertii ordinis', Whiteside describes Newton at that time as

'grown increasingly suspicious - not without good reason - of the shallowness of Gregory's mathematical insight and judgment'²².

To support this description, Whiteside cites Newton's 'diplomatic silence' over Gregory's difficulties with the catenary curve²³. Other occasions, too, such as their discussions of the solid of least resistance or the brachistochrone, must have persuaded Newton to revise his opinion of 1691. However, Newton needed his disciples, even if their abilities did not match those of the continental followers of Leibniz, and he never disparaged Gregory's abilities publicly. The faith of such as Flamsteed could remain intact.

²¹ Flamsteed to Thornton 18.2.1703 Baily op cit (16) 747-8 p.748.

²² MPVII 567.

²³ MPV 522 n7.

Then, in 1704, two others of Newton's protégés, Halley and de Moivre, pointed out the error Gregory had made in his Astronomiae over Cassini's orbit. On Newton's advice Gregory made his retraction as discreet as possible, but it cannot have escaped the attentive reader of the Transactions who had studied the Astronomiae. Certainly, the error and its retraction must have been known to the small group of scientists around Newton.

Perhaps it was this specific error, or that over the catenary, which clouded Gregory's reputation in this group. Perhaps their attitude simply reflected Newton's growing disillusionment. Certainly in the second decade of the eighteenth century, shortly after Gregory's death, we find three of the younger Newtonian scientists expressing doubts of his abilities.

Roger Cotes (1682-1716) was the brilliant protégé of Richard Bentley who was made Cambridge's Plumian professor in 1706 and edited the second (1713) edition of Newton's Principia. William Jones (1675-1749) had been permitted by Newton to produce a volume of his tracts. This Analysis had been published at London in 1711. Nicolas Saunderson (1682-1739) was not as close to Newton as Cotes and Jones. Yet, although blinded as a baby, he had lectured at Cambridge from 1707 on the Newtonian philosophy and in 1711 became 4th Lucasian professor of mathematics. He was, therefore, a colleague of Cotes. All three were some twenty or so years younger than Gregory and formed part of the second generation of Newtonian disciples.

On September 11th, 1711, Cotes wrote to Jones asking him if he could acquire Mouton's Observationes which Gregory had recommended

in proposition 25, book 5 of his Astronomiae. He said

'Though I do not much rely upon the Doctor's
recommendation, yet I should be glad to see
the Book ...'²⁴.

This book contained a method of establishing tables of numbers whose law of formulation was known by using successive numerical differences. Gregory had mentioned it as an example of the use of such techniques after discussing the determination of a comet's place by calculating successive differences. This had been discussed by Newton in the Principia, but he had given a much more satisfactory method based on his interpolation formula. Jones accordingly sent Cotes Mouton's book, but said

'tho it will only satisfy you that Dr. Gregory had
but a very slender notion of the design, extent
and use of Lem. 5 Lib. 3 of the Principia'²⁵.

It was in this proposition that Newton had given his interpolation formula, and the Astronomiae bears out the justice of Jones' remarks.

Saunderson wrote to Jones on February 4th, 1714, about proposals that had been made at Cambridge for publishing Gregory's Notae.

²⁴ Cotes to Jones 11.7.1711.

S.P. Rigaud Correspondence of scientific men 2 vols (Oxford 1841)
i 259-60.

Biographia Britannica (London 1757) is wrong in asserting 4 2372
that this letter was written to Collins and published in Cotes
Mensurarum (Cambridge 1722).

²⁵ Jones to Cotes 25.10.1711 J. Eddleston (Ed) Correspondence of Sir
Isaac Newton and Professor Cotes (London 1850).

Nobody there could give any account of them, and he asked Jones whether they explained all, or only part, of the Principia. He continued

'I shall be glad to know what assistance Dr. Gregory has had, because it may be questioned whether Dr. Gregory (though no inconsiderable mathematician) was equal to a work of this kind'²⁶.

These comments show that although Gregory's work was well known to them, his abilities were not over-rated. Cotes and Jones, with, of course, Edmond Halley, and others such as Brooke Taylor then formed the core of the scientific élite which Newton was establishing. His men were placed in academic and government posts until the science of Britain was in the hands of Newton and his disciples²⁷. Others such as Henry Pemberton, James Stirling and Colin McLaurin would soon join them. These men no doubt shared the view of Cotes, Jones and Saunderson; Gregory was a competent enough mathematician in many ways, but was not skilled enough to comment on the mathematical aspects of Newtonian philosophy.

We have, then, three groups of those who read and studied Gregory's work. There were the Scottish students who read his Edinburgh lectures, the more advanced students who studied the exposition of Newtonianism in the Astronomiae, and perhaps the Notae and Fluxions, and finally the élite of Newtonian scientists, who had read some or all of these works but regarded them with suspicion. This last group,

²⁶ Saunderson to Jones 4.2.1714 Rigaud op cit (23) i 264-5.

²⁷ Frank E. Manuel A portrait of Isaac Newton (Cambridge, Mass. 1968) 264-91.

of course, would probably have belonged previously to one of the other two.

The largest group of these may well have been the Scottish students who learnt from Gregory to understand telescopes and mechanical contrivances, to plot the paths of the stars and, above all, to survey. The mechanics they learnt made no mention of Newton's three laws, but gave them a version of Descartes' ^{definition} ~~law~~ of motion. The optics did not discuss Newtonian theories of light and colour, but gave the Cartesian approach to reflection and refraction. The astronomy did not mention universal gravitation, and barely touched on Kepler's laws, but was set in a universe of vortices. The most popular lectures of all, on practical geometry, treated a topic to which Newtonian or Cartesian doctrines were irrelevant. Gregory's individual students between 1687 and 1691 may have caught some of his enthusiasm for Newton's work, but the eminently practical, lucid, systematic lectures which were studied after his removal to Oxford could only have hindered the acceptance of Newtonianism in Scotland. They were perfectly geared to Gregory's conception of the requirements of his average student; which did not include a knowledge of Newton's doctrines.

The more advanced students in the second group, however, had different aims, and the Astronomiae must for very many have been the only suitable introduction to mathematical astronomy in the Newtonian cosmology. We do not know how many read the Notae and Fluxions, but these manuscripts, too, must have helped their readers to understand Newton's work. The scientific élite of mathematicians and astronomers in the early eighteenth century was not wrong in finding fault with these works, generally arising out of Gregory's failure to understand

Newton. Yet they were in the main competent works, quite adequate for those who did not aspire to this élite.

In minor ways Gregory might influence the path of Newtonian science after his death. The clearest example of this is the way in which his use of equant devices was adopted by later authors in the Newtonian tradition (see 5.8). Yet his mathematics was mistrusted by the core of Newtonian scientists who formed this tradition, and mathematics underlay almost every piece of Gregory's scientific work. He thus had no major part to play in moulding this tradition.

It is symbolic that when McLaurin published in 1745, the work which was to be the most popular of all Gregory had written, it was not his Notae, nor his Fluxions, not even his discussion of gravity in the Oxford lectures. Instead, he published Gregory's lectures on practical geometry and these, competent and elementary, achieved a popularity which far outshone that of the Astronomiae, Gregory's difficult masterpiece, which he had expected to outlast the sun and moon.

Select Bibliography

David Gregory's manuscripts are listed in appendix 1 to P.D. Lawrence The Gregory Family (Aberdeen University Ph.D. thesis, 1972) and I have described them in the introduction. The following abbreviations have been used in referring to their locations:

AUL Aberdeen University Library
Bod. Bodleian, Oxford
BM British Museum, London
ChCh Christ Church College, Oxford
CUL Cambridge University Library
EUL Edinburgh University Library
NLS National Library of Scotland, Edinburgh
PRO Public Record Office, London
RS Royal Society of London
SUL St Andrews University Library

The following abbreviations refer to the manuscripts:

A EUL MS Dk 1.2¹
B EUL MS Dk 1.2² and Dc. 1.75
C EUL MS Dc 1.61
D EUL MS Dk 1.2²
Misc EUL MS Dk 1.2²
CCC EUL Colin Campbell Collection
E ChCh MS 346.
RG Gregory manuscripts, RS MS 247

Items in A, B, C and D are referred to by Gregory's index numbers, and in E by page number.

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